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MY LIFE
WITH THE
MICROBES

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To My Grandchildren

Nan and Peter

NATIVE AMERICANS

YOUR GRANDPARENTS came to this country as pioneers to help build a new world. Just as earlier pioneers who came to clear the forests, cultivate the virgin land, fight the undesirable animals and transplant the desirable ones, so your grandparents came to avoid persecution, to find greater freedom, and to contribute their share in making this country a better place in which to live. They came from an old race, one that has given the world its highest code of ethics and morals; they, in turn, have tried to create more knowledge, to help alleviate human suffering, and to make the life of man a happier one. They have labored so that you will find the world perhaps somewhat freer from prejudices, freer from suffering, than they themselves have found it.

Contents

| | |
|---|-----|
| PREFACE | ix |
| 1. My Life with the Microbes | 3 |
| 2. The Town in the Steppes | 14 |
| 3. The Family and the Environment | 25 |
| 4. Memories of Childhood and Youth | 38 |
| 5. In Search of a Future | 50 |
| 6. The Promised Land | 66 |
| 7. California Interlude | 88 |
| 8. My Position at Rutgers | 102 |
| 9. Europe Revisited . . . Grand Scientific Tour | 120 |
| 10. Broadening Horizons | 157 |
| 11. Men and Microbes | 173 |
| 12. The Humus Period | 182 |
| 13. The Second World Catastrophe | 196 |
| 14. In Search of Antibiotics | 208 |
| 15. The Road to Streptomycin and Beyond | 222 |
| 16. Europe after the Second World War | 243 |
| 17. Antibiotics Take the Stage | 274 |

| | |
|---|-----|
| 18. In Spain and Elsewhere, 1950 | 286 |
| 19. Entering the Zenith | 301 |
| 20. Circling the Globe—Visiting Japan | 313 |
| 21. Practical Aspects of Scientific Endeavors | 328 |
| 22. As I Look Back | 342 |
| POSTSCRIPT—What of the Future? | 357 |
| SOME BASIC TECHNICAL TERMS USED IN THIS BOOK | 360 |

Preface

THE CEREMONIES of the dedication of the Institute of Microbiology are over. All the laudatory addresses have been made. All the honors have been conferred. The prominent visitors from all corners of the earth have departed. The Temple dedicated to the study of the smallest of living things, the microbes, has been built and equipped. The High Priests and their attendants are being assembled. They are to devote their lives to the study of these microbes. An idea has thus grown into reality.

How did this all come about? How did the humble searcher for microbes in the earth under our feet and in the seas around us, one whose primary concern was the role of infinitesimal living things in the complex cycle of life on this planet, succeed in bringing this about? This is the story of my life, the life of an immigrant boy who came from the steppes of the Ukraine to this new world in search of a better education, of better opportunities to do what he wanted to do with his life.

This life has been far from smooth—at times some rather rugged stages had to be passed. These stages were not always logical and often even appeared illogical; they were far from easy, often difficult and even strenuous. Nevertheless, as I look back over these years of my life, I feel that they have passed as they should have, as I might have expected them to pass without a serious break.

In attempting to write down my impressions, often but faint recollections, of childhood and boyhood, of youth and maturity; in selecting

those events that appear to me worth reporting here, I am tempted to emphasize those that have seemed to create the continuity of a single lifetime and neglect or pass by as superficial those that do not appear to contribute directly to that stream of life which led from boyhood to maturity, of a youth in search of an ideal, to the final attainment of that ideal. As I look back upon this past life, as I find that it was not barren, as I realize that every step was hewed out of rock, one leading to the next, and finally as I observe the attainment of a goal set many years before, in a totally different environment and under a totally different system of life, I am tempted to present my own philosophy of life and of the world at large.

What, then, were the dreams of a youth born under a despotic form of government, educated in a primitive system of schooling, struggling hard for the attainment of every minor object, on a long and strenuous path toward a goal which appeared at times to be quite unattainable? Was the goal only the search for knowledge? Was it the search for an ideal society? Was it the desire to unlock the still-undiscovered secrets of nature? It is easy enough now to look back and select those facts that have contributed to the realization of that ideal. You might say that had the course of events taken another turn, the final outcome might have been quite different, or even conclude that the final outcome would have been the same, but would have been reached by other paths, merely modified by a different environment, a different set of conditions.

Here is the story of a life of a searcher for knowledge. You, the reader, may draw your own conclusions. It is difficult to be impartial in interpreting the facts of one's own life. My interpretation is probably subject to various errors; this is only human. If any omissions have been made, they were arbitrary and were often a result of a feeling that they would have contributed but little to the understanding of my development, or because they have become so vague and so deeply buried in my memory with the passage of time that they could easily have become colored and thus had better be omitted.

My life and my work are so intimately bound together that it would be difficult to treat them as separate entities. This is why some

of the chapters deal primarily with one, and some are largely devoted to the other. Since I was born and spent my early years in a small town, surrounded by fields and forests, I had ample opportunity to observe the growth of plants, their awakening in spring, and their death and disintegration in autumn. Many a time, as I followed the plowman turning the furrow, I would bend and smell the soil, pick up the worms and admire them, then watch the young seedlings appear. As I watched my father wash his hands before every meal, rendering thereby a prayer, as I watched my mother light the candles on Friday night and pray to the Almighty, never failing to include her first-born, as I studied the Bible and the Talmud and had impressed upon me by my mother the infectious nature of disease and the need for cleanliness and personal hygiene, I became gradually aware of some fundamental principles involved. Finally, when at the age of nine, as I watched my little sister Miriam suffocate from the effect of diphtheria and as I listened to the small-town doctor tell of some great discovery, made only five or six years before, of an antitoxin that could have saved her life, my youthful mind began to wonder. Little did I dream at that time that my own life would contribute to the solution of some of these problems and to the salvation of other children.

I owe a debt of gratitude to many who have helped me with wise counsel, who have encouraged me in periods of depression, who have supported me in time of need. First, to the comrade of my youth, Peisi, who was my inseparable companion through the formative period of my early life; then to Mendel, who taught me the rudiments of plant life and how things grow; then to Dr. Jacob G. Lipman, Dr. Byron D. Halsted, Dr. Brailsford T. Robertson, and Dr. Jacques Loeb, among my teachers and counselors, who advised me wisely; to my numerous students who have helped me in the solution of the most difficult problems; and, finally, and most of all, to Bobili, my constant companion, who has encouraged me always, and who has shared with me the numerous hardships that a life such as I planned for ourselves was bound to involve.

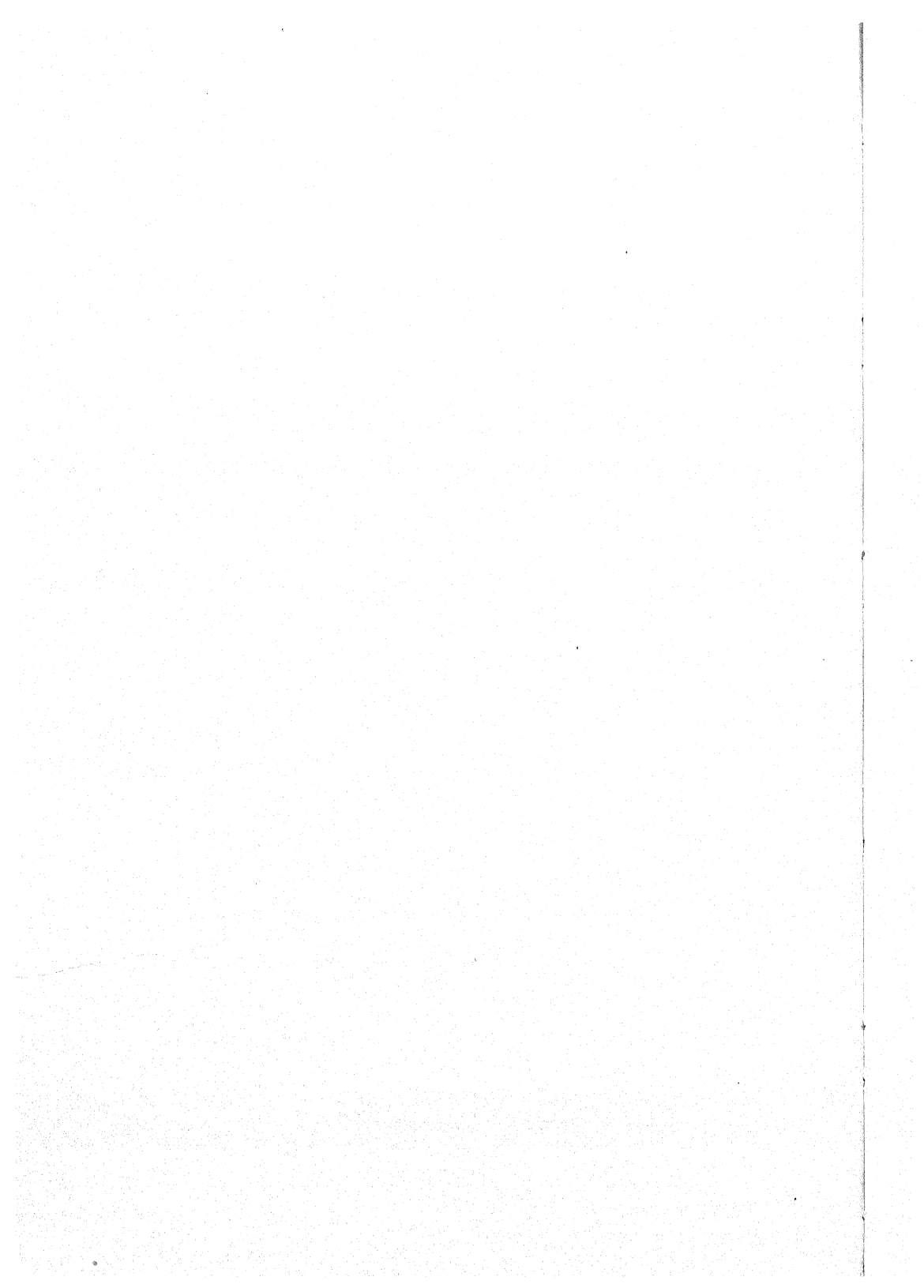
If there is any justification for one's recording the impressions gained from a lifetime of contact with people in various parts of the

world, I feel that there is certainly one in this particular case, for by choosing to devote the major part of my life to the study of microbes, I chose a career that led me into close contact with some of the greatest truth seekers of our time. If some of their dust of unselfish devotion to an ideal has clung to my own shoulders, I am grateful. If these pages make more understandable, even in a small degree, how a thirst for knowledge may predicate a successful life, I shall consider my purpose fulfilled.

S. A. W.

June 10, 1954

MY LIFE WITH THE MICROBES



My Life with the Microbes

I HAVE devoted my life to the study of microbes, those infinitesimal forms of life which play such important roles in the life of man, animals, and plants. I have studied their nature, life processes, and their relation to man, helping him and destroying him. I have measured the growth of microbes and their multiplication, their feeding habits, and the waste materials they produce. I have attempted to determine many reactions brought about by microbes in their natural environments, first in the soil, then in the sea, in peat bogs, in manure piles, and in numerous other natural substrates. I have contemplated the destructive capacities of some microbes and the constructive activities of others. I have tried to find ways and means for discouraging the first and for encouraging the second. I have worked hard to utilize the useful microbes, known as saprophytes, for the destruction of the harmful forms, or the parasites. I have studied the nature of the reactions involved and duplicated them in the laboratory for the purpose of producing chemical substances which could be utilized for treating human diseases that could never before be subjected to treatment and for improving the health and welfare of man.

Is it presumptuous for one who has occupied himself for four decades with microbes to try to evaluate their role in the life of man and what can be done to direct their activities for the benefit of man? Microbes affect man's life in many ways, from the day of his birth to the day of his death, and even thereafter, since they attack and destroy his mortal remains. Microbes are always with us, in our food and on our bodies, in our clothing and in our habitations, in the soil under

our feet, and in the water we drink and bathe in. They are always ready to help us or to destroy us. Only circumstances decide which it shall be.

Microbes are, therefore, often classified as *bad* ones and as *good* ones, as useful ones and as harmful ones. I once appeared before a Senate committee that was considering the organization of a foundation for the support of science. When I identified myself as one whose work had to do with microbes, the presiding Senator remarked that I must no doubt hate them all. My reply was that I dealt primarily with good microbes. To this the Senator exclaimed: "I did not know there were any good microbes; if that is true, only the bad ones come my way."

The prevailing impression is that all microbes, or *germs*, as they are often called, are the enemies of man. Are they not the causes of numerous infectious diseases, of pestilences and epidemics? Are they not largely responsible for the destruction of man's flocks and his crops? Have not such ravishing diseases as pneumonia and typhoid, plague and cholera, typhus and smallpox, tuberculosis and infantile paralysis, influenza and yellow fever, impressed themselves upon human history as the great scourges of mankind? Have there not been instances when a single microbe has been responsible for the eradication of a crop? This may have resulted in a devastating hunger and may have led to mass migration of the country's inhabitants, as in the famous potato famine in Ireland a century or more ago. I well recall how my grandmother, in telling me stories of great historical events that occurred in her early life, such as the sequels of Napoleonic and other wars, dated those periods as prior or subsequent to the great cholera or plague epidemics that swept the country when she was a child.

It is usually not recognized that for every injurious or parasitic microbe there are dozens of beneficial ones. Without the latter, there would be no bread to eat or wine to drink, there would be no cheese or beer, no fertile soils and no potable waters, no clothing and no sanitation. Without the useful microbes, the earth would have long ago become covered with a layer of human and animal refuse. One can visualize no form of higher life, whether that of man or animals, without the existence of the microbes. They are the universal scavengers,

they destroy waste materials, they keep in constant circulation the chemical elements which are so essential for the continuation of plant and animal life.

The older concept of *the teeming earth* has been fully justified by a better understanding of the microbes and their mode of life in the soil. Although every particle of earth carries countless numbers of organisms, these are not evenly distributed throughout the earth mass. Some are found attached to the solid particles, others live in the free water in the soil, still others prefer the tiny air spaces between the particles. Many of the microbes show special preference for the fine roots and rootlets of growing plants. A few are even capable of entering the plant roots and establishing close associations that are either injurious to the plants or beneficial to both the plant and the microbes. The dead residues of plants and animals are particularly favorable habitats for many microbes. These residues are gradually transformed into "humus," which is most important for making the soil a suitable medium for the plants to grow in.

The contacts of man with microbes are numerous. At every stage of his existence, man has become closely dependent upon the presence and activities of the microbes; he is helped by many and is destroyed by some. He often swallows them in his food, as in the case of sour milk, cheese, and other milk products. He depends upon their activities for the preparation of his clothing, his beverages, and his crops. Numerous other materials essential in the life of man, ranging from food preparations, enzymes, and vitamins to a variety of industrial products, are formed by microbes. On the other hand, man has to battle constantly against some of the microbes, which destroy his clothing and his houses, contaminate his waters and his foodstuffs, or cause infections of man and of his domesticated and wild animals, of his cultivated and uncultivated plants.

The existence of microbes is taken for granted. Most of us give them little thought. Their occurrence is recognized in a general sort of way and soon forgotten. Only very few scientists, even among the students of the microbes—the bacteriologists, the mycologists, the microbiologists—are troubled very much by what the useful microbes do and how their activities can be controlled or modified to the common good. We worry far more about the *injurious* microbes, since these

cause us harm and we have to pay a high price for the alleviation of pain.

Having received my early education in an agricultural college, having worked most of my life in an institution devoted to the improvement of soils and crops, I have been concerned with the study of the microbes in the soil, where their occurrence and activities affect the growth of plants, cultivated and uncultivated. I have been concerned with the good microbes. I have gradually learned, as have many others, that such microbes can also be utilized for purposes other than mere soil processes. Among these, their capacity to attack harmful organisms has become of very great importance.

Every war has brought with it plagues and epidemics. During World War II there was considerable fear of a new type of weapon, bacteriological warfare. Microbes, ranging from invisible viruses to some of the larger forms comprising bacteria and fungi, as well as some of their toxic products, can be utilized for combat purposes. Considerable thought and study have been given to this problem. Fortunately, this danger did not materialize. But the investigator feels certain that just as he has learned to fight most infections by treatment with therapeutic agents, so he will learn to protect the civilian and the military population of the country from enemy attack by microbes. Diseases that used to ravish the human race in former times, such as smallpox, typhoid, yellow fever, plague, typhus, and cholera, have now retreated into the limbo of history. We hope that in time other diseases, which are at present among the most serious scourges of mankind, will follow. Meanwhile, the student of microbes, the microbiologist, has made great progress in learning to domesticate the useful forms and make them work for the benefit of man, to increase his crops, to improve his foodstuffs and beverages, to yield industrially important materials, and finally to combat his enemies, the parasitic and other injurious types.

In recent years, the microbiologist has made another important contribution to human welfare by making available to the medical profession the antibiotics, whereby he utilized the inherent capacity of certain microbes to combat others. Such antibiotic-producing microbes, largely occurring in the soil, have now been harnessed or domesticated and made to render another great service to mankind. Pow-

erful drugs have thereby been placed in the hands of clinicians for combating numerous diseases that have attacked man since time immemorial. Such diseases as pneumonia, typhoid, tularemia, plague, whooping cough, brucellosis, and even the "great white plague," tuberculosis, are now subject to control through the action of antibiotics, alone or in combination with certain synthetic chemical compounds.

Man has carried out "chemical warfare" against disease-producing and otherwise injurious microbes for a long time, but it was only recently that he learned to fight such microbes outside and inside the human and animal body. Not so very long ago, it was considered difficult to poison a microbe without poisoning the body in which it lived. First, quinine for the treatment of malaria, emetine for amoebic dysentery, arsenicals for syphilis, later the sulfa drugs for many bacterial diseases, and now the antibiotics have pointed a way to final control of human and animal diseases.

By utilizing *good* microbes, the scientist has found a way to destroy the *bad* ones and has aroused hope that eventually most of the disease-producing organisms will be brought under control. Man will thus have gained full mastery over nature; he will have learned to survive in his battle with the injurious microbes. But incessant vigilance will be required to keep them under complete control.

In the study of human and animal diseases, as well as in industry, the microbiologist is concerned with single kinds of microbes, or with *pure cultures*. Only one type is capable of causing typhoid fever, another is responsible for gas gangrene, still another for influenza, or polio, or scarlet fever, or diphtheria. The same is true of industrial fermentations: only certain kinds produce vinegar, others form alcohol, still others are able to change sugar to citric acid; one produces beer and another ferments wine, the different flavors of which are due largely to the particular strains which are responsible for the fermentation in question.

In the soil, in the manure pile, and in the sea—those natural substrates for microbial development which I have studied most of my life—the microbes do not live alone, or as pure cultures, but as *mixed populations*. Every particle of soil contains many millions of microbes, representing many thousands of kinds or groups. Each of these is represented in every soil by various subgroups, or genera and species, to

which the microbiologist has given a variety of different names, for the convenience of recognition. Thus, one hears of the genus *Bacillus* among the bacteria, *Streptomyces* among the actinomycetes, *Aspergillus* or *Penicillium* among the molds. Each of these groups comprises many individual species and varieties that differ greatly from one another in their microscopic appearances, manner of growth, physiological properties, biochemical reactions, and role in natural processes. The microbes that live in the soil exist there side by side, without destroying one another completely and without one kind's becoming predominant in suppressing all the others.

I often raised the question, as no doubt many others have done: Numerous as they are, living in such close proximity to one another as they do, how are all these microbes able to get along together? Do they fight one another occasionally, or do they help one another as good neighbors? Do they compete for food, or do they live upon one another? Do they compete for space to live in and crowd out one another?

These and other questions have disturbed me as I have studied these microbes, as I watched them multiply and carry out their manifold activities, as I bade one after another come forth, at my call, and present itself for my examination. As I looked upon the wiggling bacteria under the microscope, as I saw the filaments of molds and actinomycetes come in close touch with these bacteria, as I observed the protozoa entering the picture, either consuming the bacteria or leaving them alone, I could not help but be amazed at the wonders of nature. Here in a tiny drop of water, in a minute fraction of soil, were more living things, with a greater variety of forms and kinds, than there were people in a great metropolis like New York City. Still each one went about its business of living, apparently without destroying or even disturbing the others.

As I watched under the microscope a drop of broth inoculated with a very small particle of soil, I was able to observe the following sequence of microbes, depending on the chemical composition of the broth and the concentration of the nutrients. When there was enough food, the microbes all grew and multiplied up to a certain point. Then they began to die off, some more rapidly than others. When the amount of food was limited, growth was scarcer, but it lasted longer;

in that case, there was apparently no injurious effect upon the survival of the microbes. When only a certain special kind of food was present, which not all the microbes could use, as paper or cellulose, only those that could use the particular material grew, whereas the others either failed to develop at all or made only a limited growth and then tended to disappear.

Thus, in the sequence of microbes, one followed another, each with a purpose, each able to do what the others could not do. Some were favored by others; some were injured or even devoured by others. Often one kind of microbe produced materials that tended to change the conditions of the medium in which it lived, by turning it acid or alkaline or by reducing the oxygen concentration. These changed conditions, in their turn, affected the lives of other microbes, allowing some to develop more rapidly and repressing others.

At first sight, there appeared to be no purpose or reason to the whole process, but rather a certain kind of anarchy. The stronger take all, and the devil take the hindmost. But as I learned to untangle the various relationships between the individuals in this mixed population of microbes, I began to observe certain governing principles. There was a constant adjustment of various microbes to a new environment, to a new source of food, to changing conditions, so that they could survive and continue their life activities. After all, when a microbe is unable to multiply, it soon dies. Even the mechanism of its death proved interesting. It does not just shrivel up and die. Its body seems to dissolve into, one might say, nothingness. This is not true, of course. It lyses—the visible body disappears and is transformed into water-soluble substances, which can no longer be seen under the microscope, and into certain amorphous or disorganized materials.

Among the various chemical substances produced by some microbes which influenced markedly the growth of others, two special groups of compounds appeared to be of paramount significance. These were destined to exert a great influence upon the further development of the whole field of microbiology and to find undreamed-of applications in the improvement of human health and in the control of disease. These substances included, on the one hand, the growth-stimulating compounds, which came to be known as *vitamins*, and on the other hand, the growth-inhibiting compounds, which I proposed to

call *antibiotics*. What were these compounds? What was their function in the growth of microbes? Why were they produced? How could they be utilized in combating human and animal diseases and in improving human health?

These questions must have puzzled many other students of microbes. To one accustomed to dealing specifically with the complex microbial population of the soil and of the sea, they appeared to be especially pertinent. I could well think of the sequence of types in a given population of microbes. How much similarity was there between this sequence and that of human populations? To what extent were such influences effective throughout history in the great migrations of peoples, in the appearance and disappearance of new races of mankind, in the survival of some hardy groups, in the profound effects exerted upon all subsequent human history by certain civilizations and ideologies of some of these groups, in the destructive influences of the great scourges of mankind, notably plague, cholera, and other epidemics? What similarities could one find here between microbial and human populations? After all, they are both biological or even sociological entities, both controlled by the environment, the supply of food, and the *good will* (vitamins) and *ill will* (antibiotics) of some of the constituent members of the population. Will the time ever come when human populations will make use only of the elements of good will and leave the elements of ill will to destroy the *cankorous*, or injurious, members? Or is it possible that mankind, except by the use of force, is no more able to control its *bad* members than are microbes? God forbid that this should be the answer, since it leads along the paths trod by totalitarians.

But let us return to the antibiotics. We may leave, for the time being, the vitamins, since, after all, I have been concerned with these only indirectly and have devoted the last years of my life to the antibiotics.

When I recommended, in 1939, to the director of the Experiment Station that I be permitted to apply my efforts, and that of my small group of students and assistants, to the exploitation of the possibilities of utilizing soil-inhabiting microbes for the control of infectious diseases, I was impelled to do so by three factors: first, my own observations just recorded; second, the fact that only recently one of my former

students, René Dubos, had demonstrated that certain soil bacilli could be made to produce chemical substances which had a remarkable effect upon some of the disease-producing organisms; and, finally, my awareness that a new world war was brewing and that there would be great need for new methods or agents to control infections and epidemics. When the director gave his full approval to my suggestion, I began at once to study the effects of soil-inhabiting upon disease-producing microbes, and the means for isolating the chemical compounds, or the antibiotics, produced by them.

In addressing the National Academy of Sciences in Washington, in 1940, I said:

Bacteria pathogenic for man and animals find their way to the soil, either in the excreta of the hosts or in their remains. If one considers the period for which animals and plants have existed on this planet and the great numbers of disease-producing microbes that must have thus gained entrance into the soil, one can only wonder that the soil harbors so few bacteria capable of causing infectious diseases in man and in animals. The soil was searched for bacterial agents of infectious diseases, until the conclusion was reached that these do not survive long in the soil. The cause of the disappearance of these disease-producing organisms in the soil is to be looked for among the soil-inhabiting microbes antagonistic to the pathogens and bringing about their rapid destruction in the soil.

Since then, fourteen years have passed. The antibiotics, a name which did not exist at that time, have made history. They have revolutionized medical practice. Diseases once highly resistant are now subject to chemotherapy, or, as many say, *antibiotic therapy*. No wonder that many have suggested that this age be designated as the *age of antibiotics*, rather than the *atomic age*. The control of all infectious diseases of man and of animals is now virtually within sight.

My own laboratory has taken an important part in the development of this new field of science. My knowledge of one group of microbes, the actinomycetes, to the study of which I had devoted a quarter of a century or more, proved to be of great help. Since the re-discovery of penicillin in 1940, almost all the important antibiotics have been found in cultures of these organisms. Here, for instance, is a list of some of the more important antibiotics discovered in my own

laboratories, the great majority of which are produced by actinomycetes. First came actinomycin in 1940; clavacin, fumigacin, especially streptothricin in 1942; streptomycin in 1943; grisein in 1946; neomycin in 1949; streptocin and fradycin in 1950; ehrlichin in 1950; and candicidin in 1952. Of these, streptomycin has become most famous, since it made possible the treatment of numerous diseases, notably tuberculosis, never before subject to therapy. Neomycin has also shown considerable promise. More important, aside from their practical applications, these studies opened a new field in antibiotic research by pointing to the great potentialities of actinomycetes as producers of antibiotics and to the use of well-planned screening programs for such purposes. This led to the isolation in other laboratories of numerous valuable antibiotics, notably chloramphenicol and the tetracyclines, which have since become important chemotherapeutic agents.

This work was crowned with universal recognition. I began to receive invitations to deliver addresses from all corners of the world. Most of these, I had to decline. However, degrees and awards, accompanied by laudatory comments, were showered upon me. Who was I to receive all this acclaim? To be sure, I did what was expected of me, but somebody else might have done the same thing had he been in my position. Like most of us, I can look back to numerous mistakes in my life. At times I was overenthusiastic. For this, my early education and environment may have been responsible. At other times, I may not have responded quickly enough, or may have failed to react as I should have.

But of one thing I am certain, of my life with the microbes. With them, I spent my days and my nights. To them, I devoted my energy and my thoughts. It is to them that I came in times of despondence, when I felt that the world had turned against me. It is to them that I came with my joys, to share with them my glory and my accomplishments. I lived with them, I almost shared their lives and activities. As I watched the microbes living in soils and in waters, I tried to make them last longer, go farther, and bring forth better results for the benefit of man. As I thought of the destructive microbes, whether those which affected our food and clothing, our paper and our wood-piles, or which attacked our very bodies, I was dreaming of means to keep them under control, to prevent their further spread and, if pos-

sible, to make their existence impossible, and finally to destroy them.

But at what stage of the study of microbes did the practical turn come? Was the discovery of streptomycin, for instance, the result of a definite plan, or did it just happen? Did it come without forethought, without preparation? Why did it come at a particular moment and to a particular individual?

To emphasize the significance of the discovery of streptomycin, let us look at two antibiotics reported prior to it. Penicillin was first observed by Fleming in 1929. It was rediscovered by Florey, Chain, and associates in 1940. But, to establish its usefulness, production, and nature, the United States Government is said to have spent hundreds of millions of dollars. Tyrothricin was discovered by Dubos, with all the facilities of a great institute at his disposal, in 1939, but it has found only limited practical application. There were other antibiotics of lesser importance. Streptomycin was discovered in a small laboratory devoted to the study of soil microbes. Its tremendous development is indicated by the fact that the present production has reached nearly a hundred million dollars a year. These facts have a very practical appeal.

But how did all this come about? It could not just happen at a particular place and at a particular time without special reasons.

Since my own life and my own work have been closely knit with this development, an analysis of them may help to throw some light on these questions. A great chemist once said to me: "One who attempts to write about his own life must be lacking in a sense of humor." This is no doubt true. But one must attempt to analyze the course of scientific events. This is the story of how a certain event in the history of science came to pass.

The Town in the Steppes

IT WAS a hot summer day. The large feather bed in which my mother was lying occupied nearly half of the small bedroom in the wooden, mud-covered, straw-thatched house. She could hear her own mother heating water in the adjoining kitchen. She listened to her many sisters and nieces conversing quietly in the front room separated from her small bedroom by a large clay stove, which was used in winter to heat the humble dwelling that she herself had built only a few years previously. She was at peace with the world. She was to have a child finally, after all these years of waiting, waiting. She was hoping that it would be a son. She would now have her own Kaddish, who would render the prayers for her after she died. She would dedicate him to the study of the Torah. He would be a man of learning; he would be a pride to her and to her people.

She was no longer young, by the usual standards. She had married late, when she was almost twenty-one. Had she not had to help her own mother in business and in marrying off a large brood of daughters, of whom she was the youngest? Had she not helped to bring up many nieces and nephews and see some of them married and settled safely to a livelihood? She had hardly had time for herself. Finally, when she was almost on the point of becoming an old maid, a disgrace to the family and the community, she decided, at the constant urging of her mother and sisters, to take a man unto herself. She was prevailed upon to marry an orphan, the son of a well-to-do coppersmith who had died when the boy was still a child and who had left him in the care of a stepmother and several elder sisters, a man with some property but

not much of a profession and little of real learning. She was so used to running things her own way and assisting her own mother, she would now undertake also to run her household.

She was not deeply in love with her husband. No sooner were they married than he was taken into the army, where he spent five years in military service, without seeing her even once during the whole period. By force of circumstance, she had to continue to lead her own life. She built the small house with her own means and depended upon a small dry-goods business for her livelihood. In time, her old mother came to live with her, and gradually her own house, no matter how small, became the center of the large family. She earned enough to take care of her needs and was even able to spare something for the poorer members of the family as well as the poor people of the town. She busied herself with all sorts of social problems—helping to marry off orphaned daughters, educating the children of the poor, making loans to those who needed them, though these were more in the nature of gifts than true loans, since she never took interest and often was not repaid the principal.

She was happy in her business and in her various activities, but she did want a child, a son, God help her. Her husband had finally returned, barely a year before, to her home, the one she had built and the one she managed. He spent little time with her. He had to go back and forth to the neighboring city to straighten out the various properties left him by his father, which were badly neglected. The tenants rarely paid rent; the houses were run down. Things had to be repaired. He was not experienced in such matters but did the best he could with his own hands in his spare time. He was a good artisan. Had not his father been a coppersmith, and had not the boy learned the use of tools in learning his trade? These were gone now, wasted away by his stepmother, sold off by his elder sisters. But he still had the houses that had been left to him and to him only. Since his young wife had her own business, he did not have to worry much about living expenses, so long as he had enough for his own limited needs.

He was simple in his tastes and in his demands. Army life had depressed him much. He was now satisfied to divide his time among his property, his wife's house, and the local synagogue. Here, in company with others, he would pray, study the Bible, or read some of the

other holy books. He liked best to read books of travel, of adventure, of other peoples and other worlds. The only thing he lacked was a child. Now his wife was going to give birth to his child. He too was hoping it would be a son. He would cherish the boy; he would tell him of the wonderful worlds far away from this town, away from this country.

As his wife was dozing away, between recurrent pains, barely listening, in fact, barely recognizing the faraway voices of her mother and sisters busying themselves with her, my father was in the house of worship, covered with the holy shawl, praying to the Almighty that his wife would come through in peace and that she would give birth to a son. Everybody's hopes and prayers were fulfilled. It was a son. As the news reached him, he took out with trembling hands the bottle of brandy which he had prepared for the occasion and shared it with the other nine members of the group of worshipers. He was happy to receive the good wishes. As tears streamed down his face and into his long beard, he must have thought of his own mother, whom he barely remembered, and of his own father, of whom also he had only a very vague memory.

But happiest of all was the young mother. She had borne a son, her first. She would dedicate him to God and to the study of the Book. Was she not a daughter of an ancient people, a people who, throughout the centuries of wanderings over the face of the earth, preserved two things: love of their God and reverence of their Holy Book? Both gave them hope and courage to go on, to strive for a better world, while they prayed for the time when all the dispersed children of Israel would be gathered together again in their Holy Land. The greater their suffering, the nearer would they come to the final salvation. They had suffered enough. Had she not read it herself in the books; had she not heard it told by the old and the wise of her community? Now that she had a son, things would have to be better, if only for him. She would bring him up as a good son, a pride to herself and her people. She looked at her newborn, stroked his soft cheeks, gave him her full breasts, and watched with joy how hungry he was, how happy he was to feed and to sleep.

As her family surrounded her now, blessing her son, she smiled to herself and went off to sleep. She was now full of happiness and hope.

Was not her name Fradia, which meant joy? Was not her husband's name Jacob, after one who was the father of her people? She would name her son, after her own father, whom she barely remembered, after the Great King, the greatest and the wisest of them all, the King of Kings, Solomon the Wise, a name which had become corrupted during the centuries and had gradually been changed to Zolmin.

Thus the hero of this book was born.

I heard the story of my birth repeated so often in my childhood by my mother, by my grandmother, and by my many aunts that I always see all these events before my eyes as if I had been an actual witness of them.

I was born on July 8, according to the old Russian calendar, in the year 1888, in the town of Novaia-Priluka. This town, the birthplace also of my mother and my grandmother, was in the heart of the western section of the Russian Ukraine, in the government of Kiev, the ancient capital of the region. It was some forty miles distant from the county seat of Berdichev and some twenty miles from the nearest large city, Vinnitsa, where my father and his father had been born. Novaia-Priluka, like thousands of other towns in the Ukraine, was surrounded by small settlements or villages, where the peasants lived.

It was a bleak town, a mere dot in the boundless steppes. It was flat and surrounded by wide, forestless acres. In summer, the fields of wheat, rye, barley, and oats formed an endless sea. In winter, snow covered the ground, and the frosted rivers carried the eye to the boundless horizon, where the skies met the earth somewhere far away. Only the slow-flowing rivers and brooks, with occasional groves of oak and chestnut, broke this continuity of land and sky. The earth was black, giving rise to the very name for that type of soil, *tchernoziem*, or black earth. The soil was highly productive, yielding numerous crops, grown continuously for many years, without diminishing returns.

The town itself would hardly impress a visitor as worth a second glance. It had little if anything to offer to a city dweller. It was a small town. Very few of its inhabitants obtained from life more than a bare existence, and even that required hard, continuous struggle. In spite of the abundant crops and the well-fed herds of cattle and sheep, of swine and horses, and the hard-working people, life was materially poor, since the resources went to fill the coffers of the landlords, the

Czar and his retinue, and the police. Spiritually, however, it was rich beyond description.

The people of Novaia-Priluka did not lead an isolated life. Their activities were closely interwoven with those of the rest of the world: first of all, with the life of the surrounding peasant villages and to a lesser extent the estates of the landlords; secondly, with the life of the people in the big cities; and thirdly, with the life and ideals of the country at large. This town thus formed a tiny link in a long chain of continuity of life in old Russia, a chain of which the thousands of other towns, villages, and cities made up the links, all of which resulted in the rich Ukraine, the proverbial granary of the Great Russian Empire and of Europe itself.

Just try to imagine the physical appearance of the town. Several parallel rows of whitewashed, straw-thatched adobe houses surrounded a large, open square, with a well at one corner. Here, on religious festivals or on market days, the peasants from the various villages would come to trade, to sell their agricultural produce, and to buy manufactured goods. They also came to meet friends and to make merry. Returning to their villages in their creaky, ungreased, horse- or ox-drawn wagons in summer, and in their smooth-running sleighs in winter, they left the square full of garbage and refuse. Then for several days, peasant women cleaned up the debris with their long-handled brooms made of reeds and rushes growing close to the local brook, and again the square was bare.

Many of the houses in town were attached to one another, in rows of fifteen or twenty, without the supplementary benefits of either garden or porch, or the common facilities so characteristic of small homes in America. These row houses often served partly as stores for local storekeepers and partly for habitation.

Most of the houses consisted of a large living room with a lime-washed earthen floor, a small bedroom, and usually a small kitchen, the major part of which was occupied by a thick-walled clay stove. This stove served many important functions: for baking the weekly supply of bread, for preparing the daily meals, for heating the house, and occasionally offering extra sleeping quarters for guests or members of the family. Its broad, flat, clay-coated surface would often be offered, especially in winter, as a welcome resting place to the visitor or to the

youngsters of the family. Occasionally, the house had a cellar or garret, both serving to enlarge the living quarters of the household or to provide storage space for casks of pickled vegetables, fruits, and home-made beverages.

Here and there on the outskirts of the town was a house somewhat more impressive than the others. A tin-covered roof, a slightly larger structure containing more than the usual three rooms, a wooden fence, and often an adjoining small garden were the meager niceties that indicated the wealthier inhabitants.

The villages were either contiguous to the town, or were only two or three miles away. One could easily determine where the town ended and where the village began, by the greater abundance of vegetation in the village, by the larger parcels of land surrounding each peasant household, and especially by the cows, sheep, swine, and poultry that roamed in the village and often found a place of rest in the houses themselves.

Where the village ended, the fields began. Frequently, a clump of oak, maple, or chestnut trees, a grove of willow trees along a brook, or a well-planted and well-cared-for wood lot would impinge upon these limitless plains. Occasionally a kurgan, a remnant of burial ground of the ancient inhabitants of the area, would form an earthly mound to break the monotony of the grainfields in summer, and of the snow-covered land in winter. Here and there would appear the peasant mud houses, with their small windows and wooden fences. Who could ever forget the spring and summer aromas of the flower beds, of the cherry trees, and of the apple orchards surrounding these houses and giving them for a brief time a festive appearance? In winter, they looked gray and forlorn.

The economic life of the town and of the village was closely interwoven, but not so the cultural. The inhabitants of the town and the village, the Jews and the Ukrainian peasants, were two distinct peoples, different in racial origin, in historical background, in religion, in habits and customs, in communal life, and even in their very languages. Though they formed an interdependent economic system which dominated the region as a whole, spiritually the two peoples might have lived in different worlds.

In addition to the two major cultural strains, certain minor groups,

who had their own customs, religions, and often languages, and who also lived a cultural life all their own, inhabited the region. Some of these groups were derived from earlier peoples who had once dominated the region, as remnants of Poles, Czechs, or Germans. Some of them had been brought in from other countries in an attempt at more modern colonization. Then there were the true Russian groups, representatives of the governing power. These were occasionally respected, as were some of the teachers; they were often despised, being referred to as *katzapi*; more often, they were feared, since they, especially the inspectors, the police, and the Cossacks, represented the power of the Empire. These extraneous elements usually had a limited, often superficial, influence, upon the life of the community, the town-village complex.

In picturing this socio-economic-cultural complex, we must not forget the landlords, some Polish and others Russian, who once had owned all the land and whose serfs the peasants had been until quite recently. Their estates comprised sumptuous palaces, surrounded by greenhouses and vast fields. The landlords themselves, or *pomiestchicks*, had retained most of the magnificence so characteristic of the life of the last century and described so well by the great Russian novelists Gogol, Turgenev, Tolstoy, and Goncharov. They and their families spent most of the year abroad, living either in villas along the Riviera or in summer homes in Paris, London, or Rome. They often had special residences in Moscow, Kiev, or St. Petersburg. Only occasionally would the landlords and their families come to spend the summer months on their estates, which were cared for by the underlings who gathered the harvests. It was these landlords who suffered most from the Revolution that came later. No wonder they contributed so many of the refugee groups dispersed throughout the world.

The town itself was neither agricultural nor industrial. Nor was it commercial in a true sense. It was just a town. But it was highly important to the life of the community, since the slow means of communication made the big cities seem so far away. The village was exclusively agricultural, producing crops, raising animals, supplying labor and services. The peasant brought his produce to the town market place on holidays and on special market days. He sold the produce to one of the local merchants. He proceeded directly to one of the many

stores to buy cloth for a dress for his wife or a suit for himself; he bought leather for boots for himself or for a saddle for his horse. He then visited the town tailors, dressmakers, shoemakers, and saddlemakers to have the measurements taken and the orders filled. He made extensive use of the other artisans in the town, the wagonmakers, the blacksmiths, the coppersmiths, to meet his needs. He would go to the local dentist to have his teeth fixed, to the local lawyer to have a complaint filed against his neighbor, to the local barber to have his hair cut or his simple ills attended to, to the pharmacist to buy his ointments and pills, and finally to the government liquor store to buy his vodka, which he would consume just outside the premises. The town was thus the commercial, industrial, and professional nerve center for the whole group of villages surrounding it.

The peasant could not obtain a sufficient livelihood from his crops. He depended also upon the labor that he and his family supplied to the landlord and to the townspeople. The landlord provided the land; the peasants, the labor; the townspeople, the artisans, the merchants, and the small group of professional men. This was the mechanism that kept the economic machinery of the whole region in motion.

In their religious needs, their educational requirements, and their degrees of equality or status before the law, the life of the townspeople and that of the villagers parted. The latter had simpler educational needs, but they enjoyed greater legal privileges. Not often did a peasant's son strive for an education beyond that of the primary school. But seldom did the requirements of the peasant exceed the capacities of the local priest, the schoolteacher, the compact community council. The townspeople, on the other hand, were more ambitious. They were more anxious to break the tight chains that encircled them. They were more anxious to drink their fill of the wisdom of the ages. Thus the spiritual needs and the intellectual strivings of the two peoples placed them on two different planes, whereas their economic needs and facilities fitted them into a perfect system.

The period embracing the end of the nineteenth and the beginning of the twentieth century was a most disturbing one in the history of Russia. There were much poverty and considerable dissatisfaction among the peasants, who had been liberated from slavery only a bare three or four decades before. Socialism was growing by leaps and

bounds. The Revolution was in the making. Although the masses started no open rebellions against the government, a few individuals here and there felt the need for more opportunities in education and for greater participation in the life of the country.

As is usually the case, the city intellectuals and the townspeople served to awaken these strivings within the whole population. The landlord was there, however, and the law was always there, as represented by the local police. But it was especially the power from beyond, the ruler of the land and his minions, who governed the oppressed, who established the law, and who dictated to each individual the extent to which he might strive for self-expression and for self-improvement. It was this long arm that often introduced a most disturbing element into the life of the two peoples who made up the town and village community and that prevented them from planning together, from fusing, and from expressing their joint needs. Frequently, this power went a step further and stirred up ill feelings among the villagers against the inhabitants of the town. This power was thus successful in inciting trouble between the two communities and diverting them from a direct attack upon the aggressive power itself. But this could not last forever, as history has proved.

It was in such a town and in such an environment that I was born. This town is now gone. It was swept away by another power, an aggressor far more ruthless and far more destructive than the one that dominated the region previously. The child survived the catastrophe to tell the story of his town, his people, the peasants, the government beyond, and the forces of good and of evil that finally forced him to leave the country of his birth to look for shelter in other lands, in freer surroundings, in a New World.

My feelings at the destruction of my town were expressed in free verse, in the form of a Christmas greeting sent to some of my friends in 1941:

THE SAGA OF A TOWN

*Down where the Dnieper flows,
Lay a little town,
Amid endless plains of rich black earth,
The town where I was born.*

The Town in the Steppes

23

*Its simple folk, its artisans and peasants,
Its workers and traders, toiled and prayed,
And led a peaceful and productive life,
In the town where I was born.*

*The oppressor had gone, the yoke was broken,
And the people were free, as you and I,
They looked with hope to a happy future,
Of which their labors gave ample promise.*

*Then, from the West, came a new oppressor,
As ruthless a barbarian as the world had ever seen.
In cruelty far exceeding those of olden times,
He came not to build but to destroy.*

*His army swept through the town, the peaceful town!
They killed and maimed both young and old,
They destroyed what they could not take,
And gone is the town where I was born!*

*Its straw-thatched roofs, its adobe walls,
All served to feed the raging fires,
An accurate hand, a single bomb,
And the town was no more, the town where I was born.*

*O Lord Almighty, is Thy mercy gone?
Are misery and suffering alone Thy gifts,
Are evildoers to be the rulers of this earth?
Are they to guide the destiny of man, the spirit of creation?*

*O Lord Eternal, send down Your avenging angels,
Purge the oppressor, strike him and all his
Tools, slaves, and retinue, strike the barbarian
Before he has destroyed the seed of man.*

*Let then man erect a towering shaft inscribed
That never shall another tyrant arise
To take power over his fellow man.
And let justice and righteousness again prevail.*

Four years later, this town was liberated. It is now but an empty shell. All its people are gone. They were destroyed by the barbarian in a manner that could only be compared with that of Genghis Khan and other tribes who had roamed this region centuries before. There was this difference, however. The earlier barbarians laid no claim to culture. The modern barbarians were clothed in a thin veneer of civilization that revealed far greater bestiality than that of any primitive men.

I expressed my feelings at the liberation of the town in the following verses:

MY TOWN IS FREE AGAIN

*A ripple has come across the sea,
Your town is free!
The town is free, but the people are gone,
And the land is bare.*

*The barbar saw to that,
His skin is white, but his soul is black.
A wave of destruction was left behind,
To compare with that of the Vandal of yore.*

*But my town is free!
May it be a symbol,
For ages to come,
Of man's inhumanity to man!*

*Let your child and mine
Remember my town!
And may the price that we have paid
Create a better life for them!*

The Family and the Environment

THE family into which I was born was a true matriarchate. My grandmother, Eva London, was a prominent merchant in the community. She had lost her husband, Zolmin, a tanner, when she was only thirty years old. She was left to care for a brood of eight daughters, the youngest of which, my mother, was only two years old when her father died. She had had very little formal education; in fact, she had not even learned to read or to write. She had, however, amazing mental faculties. She could remember in the smallest detail her various accounts with the petty merchants of the town, for whom she served as the commission buyer. She kept her accounts mostly on the back of a closet door, making a number of chalk marks which she alone could interpret. She saw to it that her children, especially her youngest daughter, received some education. When my mother was still young, she was already capable of helping her mother with many of the business details. My mother was an energetic person. She was not satisfied with remaining a mere assistant to her mother.

My father's father, Abraham Waksman, had been a hard-working man. We knew very little about him, because my father could hardly remember him. He must have been a very pious man, since he had devoted part of his earnings to the building of a house of prayer on his own premises. His daughters, in whose care my father was left, tried to give my father some education. He received some training as a weaver of fiber-covered chairs and other furniture. This was usually considered more or less an occupation of poor children, rather than of one who, like my father, had inherited considerable property.

His knowledge of the Holy Scriptures was also rather limited; although he could write and read well and knew his arithmetic, he could hardly be considered a learned man. It was presumed that since he had inherited a great deal of property from his father, he had nothing to worry about in life. This property, mostly real estate, was not properly cared for, although it took a considerable amount of his time in later years.

From my very early childhood, I remember myself as my mother's joy, her hope, and her pride. My beloved little grandmother and my many aunts and maiden cousins did their best to spoil me completely. In the hope that her son would grow up to be a man of learning, my mother directed my early education with a wise and strong hand. I was destined to be for seven years an only child of a mother who was the youngest and brightest of a large brood of women. I had thus every opportunity to become the center of a small universe, not only in our own household, but in the households of our numerous relatives and friends in our own town and in adjoining towns and villages. That I did not become entirely spoiled was due largely to my mother. I possessed from childhood an inquisitive spirit. In this, I was encouraged by my grandmother and my mother, who tried to satisfy as best they could my keen desire for knowledge. I was also helped in this by my father, who would tell me of his adventures in the army and read to me as often as my spare time would permit. There was a certain camaraderie between me and my schoolmasters, some of whom took special pains to encourage me to go on and on in search of more knowledge.

Of all the people who influenced me in my childhood, especially in my mental and spiritual development, a few stand out sharply in my memory. First of all were my mother and my grandmother, then several of my early teachers.

As I first remember my grandmother, she was already nearly seventy years old. She was kind, gentle, and capable. She was the typical storybook grandmother. To me, her wrinkled face was forever smiling. Her soft hands, her quiet voice, could always bring my boiling youthful spirits to a state of peace. Never did she scold me. She loved me, as only a grandmother would love the only child of her youngest and dearest daughter, with whom she spent all of her later life. Grand-

mother's face! Who could ever forget it? What child could experience greater joy than having a grandmother like her! She would listen to my early inarticulate speech, she would fill my childish ears with stories of old events in the life of the town and of the country, of wars and epidemics.

Grandmother remembered the Napoleonic war, having listened attentively to the stories of her own father, a man of learning and wisdom, who had lived in that period. She would tell many stories of the days of Chmielnicki, of the rebellions of the Cossacks against the Poles, rebellions which played havoc with the life of all the people in the Ukraine, especially the innocent Jews who were thus caught between "the Hammer and the Anvil," as so well described by a brilliant Polish writer. She herself remembered the last Polish rebellion of 1864 against the Czars, when the battles raged in our region and reached even her own home. I listened to these stories, avid for more details.

When I was ill, it was always my grandmother who attended to my needs, who often spent sleepless nights over my crib. When I came home from school or from street games, either full of joy or covered with mud and tears as a result of a street brawl, it was to grandmother that I went first. She listened with all the attentiveness that a child requires and fully participated in all my sorrows and joys.

When she came back from the big city with loads of goods for her customers, the first parcel—candied fruits and other sweets, and later toys and books—went to her beloved grandchild, who was also the first to greet her upon her return, often meeting her wagon at the entrance to town.

Grandmother had generally a very hard life. She was the daughter of a melamed, a teacher of children, but one who could hardly boast of a great excess of worldly goods. He had married off his energetic daughter to a young artisan, a tanner, which did not ensure great social prestige. My grandfather had to keep his tannery some distance from the town, to prevent the violent smells from permeating the whole atmosphere of the town and the villages. He died while still relatively young.

My grandmother, being a dynamo of activity, promptly moved back to the town of her birth and established a commission business. Each

Monday morning, she hired one or two wagons with peasant drivers, and proceeded to Berdichev, where she usually spent three days filling numerous orders for the town storekeepers. On Friday afternoon, the packed loads rolled up to our house. Grandmother would slowly climb down, to be surrounded by her daughters and grandchildren, as well as the local merchants who came to claim their orders. My mother and the others helped to unpack the wagons, and soon the merchants went off carrying their orders with them. These were all carefully checked, grandmother doing most of it without benefit of pen or pencil.

Of her eight daughters, one died in childhood and another on the eve of her marriage. All my aunts had many children of their own and lived either in our town or in neighboring towns. They frequently came to visit grandmother, bringing their broods with them. The sons-in-law and later the grandsons came to consult grandmother on all important events, ranging from business transactions to the problem of marrying off their children. Since economic conditions in our town offered little opportunity for active young people, my cousins gradually began to emigrate to the United States. One of my early recollections was the departure of my young cousin Molki, a girl barely seventeen. She was on her way to Chicago, where she would marry a young man who had left our town several years earlier and who was now able to provide for her transportation. Although the young man and his parents were well known to our family, still Molki's departure was considered a great tragedy. There was much tear-shedding as the young bride-to-be was finally sent off across the seas to the New World.

Grandmother married off her daughters well, one of my uncles becoming a prosperous merchant, another a learned rabbi, and three others traders of one sort or another.

My mother loved me dearly, with an unselfish and devoted love. I knew how much she missed me when I had to leave home to complete my education. She led me in the path of righteousness, she counseled me, cautioned me, watched over me, took pride in my attainments, and listened to my ambitions; she encouraged me, when she felt that my own courage was beginning to fail at times. I was devoted to her. I repaid her love with pride in her, with desire to live up to

her expectations. It was her death that was the main contributing cause for my leaving the land of my birth and seeking a home in a new world.

She had another child when I was seven, but little Miriam died less than two years later from diphtheria fever. A shipment of antitoxin from Kiev, some two hundred miles away, arrived too late to save my little sister's life. As I watched her die, my childish and observant mind may have speculated on the possible effect of the curative agents upon the disease and the potential salvation of her life. Here, for the first time, I was brought in contact with a type of problem that was later to receive much of my attention. I was worried and I cried. I loved my little sister! I needed her so much.

With the death of my sister, my mother spent on me alone all caresses and hopes. Although she never punished me herself and saw to it that no teacher inflicted upon me any physical punishment, she did not spoil me, but would discuss with me what was good and what was bad. Woe to the teacher who would dare lay a hand on me as punishment for some minor childish prank! This happened once, and I was immediately removed from that school and placed in another. She guided strictly my early education, both in book learning and in social responsibilities. There were no public or government schools in town. I was sent, therefore, at the age of five, to a *heder* (private school) where the *melamed* instructed me in the rudiments of reading the Bible and in rendering the daily prayers. Within two years, I was transferred to a more advanced teacher, where first the prophets, then the Talmud with all its complicated interpretations comprised most of the intellectual diet.

My mother was not fully satisfied with this type of schooling for me. Before I was ten she engaged private tutors for me. These tried, I must admit with considerable success, to lead me through the devious paths of the Hebrew and Russian languages, of literature, history, arithmetic, and geography. When I reached the age of thirteen, I had a rather thorough knowledge, not only of biblical and talmudic fields, but also of the Russian language and literature.

No greater joy had my mother than to listen in the evening to my recital of the new facts and new ideas that I had acquired during the day. My father also took part in such interrogations, but more as

a connoisseur checking on my accomplishments. Mother and grandmother would stand with smiling faces, even open mouths, and listen to my childish patter. I often recall returning home from school, having crossed the muddy or snow-covered streets, to have my excess clothing removed by my mother, her face radiating joy and appreciation as she listened to my adventures into the fields of knowledge.

Father, as a child and as a youth, had been under the influence of his sisters and learned, no doubt, at an early age, to escape their nagging at the prayer house, where he would spend his time reading books or praying. When he married my mother, he passed over from the influence of his own sisters to that of his wife and her mother. The trade that he had learned in his youth was rather an avocation, and he engaged in it when he was tired of everything else. He seldom was engaged in his work, although he had many requests for it.

My father talked with me often, his greatest influence upon me being that of a storyteller. He was full of stories of wise men, who lived mostly in ancient times, and also of important historical events in the long life of the Jewish people. He and I were good friends, ~~but~~ not comrades. We seldom played games or engaged in activities that would bring us close together. But my father filled a certain place in my life, without which it would have been much poorer. He made it broader and richer, but not deeper or wiser. He was always in the shadow and did not play that profound part in the life of my boyhood that fathers usually do in the lives of their sons.

To some of my early teachers, I owe a great deal of gratitude. They taught me, they encouraged me, they often guided me. There were a number of these teachers, ranging from those who taught me first the simple principles of reading and writing, the study of the Bible, a large part of which I easily memorized, and finally the study of the Talmud with its various commentaries. Some of these teachers were simple people. In a singsong voice, the teacher would read a word or a sentence, and the students, often as many as thirty or forty, would repeat it in unison. A yawn or any other indication of lack of attention was usually punished by striking a ruler upon the boy's hand. Some of the teachers, especially when I began to study the Talmud at the age of eight or nine, were more philosophical, or even sophisticated, in their analysis of the words of learning and the world at large.

As schoolteacher was followed by tutor, and with an increase in

the specialization of my studies, the group of students became smaller, often comprising only four or even two boys, both the tutor and the subject became more interesting, often fascinating. The classes were usually held in the homes of the teachers. These managed to earn a rather pitiful living. They had no formal training in government schools or universities. They had received an education similar to the kind that they were now imparting. Many of them had very little worldly wisdom, but they knew their subjects, they loved their work, and they were devoted to their students.

In the small classes, I usually became beloved by the teacher and often managed to carry off many prizes and honors. I recall a simple incident. One Friday, when a group of us, comprising five boys, were leaving school for a long week end, the teacher drew out a new copper coin and said to us: "When we meet again on Monday morning, the one who knows his chapter in the Bible best will get this kopek: those who do not know it will be punished." The following Monday two of the boys, in punishment, were made to kneel on a layer of buckwheat seed—the teacher lived in the home of a grinder of cereals. One of the boys passed his test, and another was praised. It was I who received the copper. I must have been seven or eight years old then. I recall the teacher's placing me between his knees and handing me the glistening coin with joy in his eyes.

My fondest memories of my childhood center around family gatherings, weddings, religious ceremonies, or simply groups of visitors who would come to see my mother and grandmother.

My mother was quite learned, especially for a woman of that period and under the living conditions prevailing in a small town. She knew Yiddish literature and even had a reading knowledge of Hebrew, or as much as was required to read the Scriptures and their simple interpretations; she also possessed a good speaking knowledge of Ukrainian.

Some of the unforgettable events in my childhood were evenings on certain religious holidays, when many women in the neighborhood would gather at our home. They would sit on the earthen floor, around my mother, with candlelight illuminating the whole house. My mother would read to them ancient stories describing the historical background of the Jewish people, especially the destruction of the Temple in Jerusalem. These women, wrapped in their shawls or in holiday attire,

would listen, often repeating or intoning after my mother, and frequently shedding bitter tears in the process. I must have been seven or eight years old. As I would enter our house on one of those nights and watch these simple semireligious ceremonies, I could not help but begin to understand how these people remained strong in their faith and in their hope of returning to their ancient homeland, the land of Israel, even after an exile of nearly two thousand years.

Among the childhood events associated with my father, I shall never forget those cold winter evenings, with the snow storming outside. While I was lying rolled up like a kitten on the large clay stove, occupying a good share of the front room of our house and heated with wood or with bundles of straw, my father would be sitting on a bench close by. His bearded face buried in a book before the lighted kerosene lamp, he would read or translate stories to me. These stories dealt with the lives of famous rabbis or other holy people, with ancient voyages across unknown lands, and with historical events. On Sabbath day his readings would be from the Proverbs and the Psalms. I would fight against falling asleep when story and fiction, fact and imagination would intermix and one could hardly tell where one ended and the other began.

About that time, other members of the family would gather around the table. Mother would soon put up the samovar, and tea would be served. Frequently conversations followed the stories, based either upon them or upon family problems, business or social problems, but seldom upon problems of a political or national character. The town was too far away from the centers of political life of the country to feel any particular repercussions. Our family circle was far more concerned with the immediate needs of its own group, or with those of the townspeople, than with the political structure of the country or with international events.

Only very few daily newspapers reached our town, usually a few copies of the Hebrew journals from Warsaw or Vilna, and a few Russian papers from Kiev or from Moscow. They would be circulated among those interested and did not arrive at our house at all—or only by word of mouth from one who had read them—until I grew up and subscribed to them myself.

Only when some world-stirring event of particular interest to the

town was taking place, such as the crowning of a new czar, the Dreyfus affair, or the Boer War, or when problems of great importance, such as the drafting of the reserves for military service, as in the Russo-Japanese War, did the town bestir itself. On the last occasion, the town became violently agitated, not so much by the war itself as by its consequences. These comprised the country-wide strikes of all the railroads and factories, the pogroms organized by the government upon the Jewish inhabitants of many towns, and finally the granting of a temporary constitution. The townspeople now felt that the time might have come for striking off the chains that had bound them for so long. Since I was then at the impressionable age of sixteen to seventeen, these events were bound to have a tremendous influence upon my own development, and finally led to my awakening from the lethargy in which I was sunk and to my leaving the town soon after.

Among the other impressions of my childhood were the market days. My mother would get up early and, with the assistance of a peasant driver and the help of my father, when he was at home, load the heavy rolls of textiles which were to be transported to the market place, usually seven to twenty miles away. She would return home late in the evening, tired and often quite exhausted. I would spend the day at school, or, when still young, at a neighbor's house or in the company of one of my innumerable female cousins.

Occasionally mother would permit me to go along with her on one of those out-of-town trading trips. This was one of the greatest joys of my childhood—the early rising, the trip on a wagon in summer and in a sleigh in winter, the exciting moments of unloading the goods at a given stall, the passing peasants in their holiday attire, visits to other stalls. My particular attraction was the many bookstalls that were always present in the market place. I was fascinated by the gaudy picture books. From there I would wander all over the market place, touch the grain in the sacks, the young goats and sheep. I would listen attentively to the plaintive songs of the wandering troubadours, who accompanied themselves on primitive harmonicas or guslas. The dances of the peasants, especially the hopak, the bargaining for the sale of a horse or a pig, the gypsy groups—all this was highly exciting, and so fascinated me that it lingered long in my memory.

All sorts of impressions remained of some of those trips to neighboring towns on market days or for special visits to relatives and friends. I recall on one occasion a heavy snowstorm that overtook us as we were returning home late one night. The driver lost his way and, after circling around for nearly two hours, decided to stop for the night at one of the peasant houses in a village through which we were passing. The owner received us in the friendliest manner and, although he and his whole family lived in only one room, the larger part of it being occupied by young goats and a brood of young goslings, he offered my mother and me the place of honor, the top of the warm stove. We were given some black bread and freshly cooked eggs, and then allowed to spend a most restful night. My mother knew well many of the peasants in these villages; they often came to trade with grandmother, for whom they had a high regard.

The combined income of my father and mother was about sufficient to keep up a modest household and provide enough to pay salaries and fees to the teachers who were engaged for my education. There was also a little left over to help a needy niece or nephew or to give to the poor of the town.

I became so impressed with the social consciousness of my mother that when I was barely twelve or thirteen years old, I began to organize a school for the education of the children of those who were less privileged. To me and my young comrades, education meant something quite different from that of the pious people of the older generation in the town. We did not teach the children prayers and Bible readings, which they studied mornings in the religious schools, but, rather, we grounded them in Hebrew and Russian grammar and in writing. We also taught them some arithmetic and some history. For this purpose we rented a room in a private house, bought some furniture, and invited the children of the poor to come every afternoon from four to six for their free education. We managed to supply these children also with books, pencils, and paper. There were four of us teachers, all boys of twelve to sixteen. We gave freely of our time, not only in instruction, but also in collecting funds that were required for the support of the school.

When, years later, long after my arrival in the United States, I would visit New York or Philadelphia and attend special gatherings

of our townspeople living in these cities, some man or woman, frequently older than I, would come up to me and introduce himself as one of my former students, who would have remained completely untutored had it not been for my youthful efforts.

The major difficulty experienced by our school, which lasted about four or five years, came from the police. They raided the school every few months, since we conducted it without proper government approval, which could never have been granted. We always had to have a few rubles on hand to buy off such raids.

My mother encouraged me to interest myself in the condition of the poor and the underprivileged. She would help me to collect money for the school or make her own outright gift. She also encouraged me to organize a group of boys to take care of the sick, since there was no resident doctor in town, and medical help was scarce. She taught me at an early age to learn the value of money and if possible earn it by working for it. Since I learned at an early age to read and to write, and since the opportunities for general or special education in our town were rather limited, I had all sorts of tempting offers from parents of the less privileged to do private tutoring of their children. Later, wealthier people in the town and even some of the richer peasants engaged me to help prepare their sons for entrance to various schools. I took full advantage of such opportunities in order to relieve my mother of the extra burden of paying for my special education. From the age of ten until the day I left Russia, I was never short of pupils, who ranged from those in elementary grades to those preparing for entrance to the university. My fees began with one ruble a month for thirty minutes or an hour of daily instruction and ended with five to ten rubles a month when I grew up and both my knowledge and experience as a teacher were firmly established. I must add that the ten-ruble fee was rather sumptuous; such sums were usually paid only by the more advanced students. Some of these students were far more advanced in their official scholastic standing than I, but they apparently had much less actual knowledge than I did, and could thus benefit from my instruction.

Other early impressions come to mind. There was the heder. I was the youngest one among the children. The melamed kept me sitting on a narrow bench, in front of him. I could thus take full ad-

vantage of his teachings. Since I lacked at home the companionship of other children, I naturally liked going to school. Because of my rapid learning and the high respect of the teacher for my mother and my grandmother, I escaped the frequent beatings given to most of the older boys.

I usually managed to keep out of the clutches of the older boys, because, on the one hand, of the help I often gave them with their studies, and, on the other, of my own circle of friends, boys from the quarter of the town where I lived. Soon we formed a small gang, with the motto "all for one." We carried on frequent battles with other groups of boys, either from our own or from other schools. The worst gangs came from the sections of the town where the artisans lived and where the boys either did not go to school at all or were limited to short schooling periods. Perhaps that was one of the psychological reasons why, at a later date, I undertook to educate these ruffians. The usual result of my encounters with such gangs was a black eye or a swollen lip. I hated to return home afterward, in fear of two things: the grief of my mother and the vengeful spirit of my grandmother. I managed, however, to pacify both; but this did not prevent me at the next encounter from meting out, especially when supported by my own gang, similar punishments upon our adversaries.

Some of the memorable events of that youthful period of early education were the ceremonies which were frequently held in connection with the initiation of other boys, first into school at the age of five to seven, then into the study of the Bible and later the Talmud, at the age of about eight to twelve, and finally the *bar mizvah* (age of thirteen). It frequently fell to my lot on such occasions to deliver a blessing upon the initiate. This comprised an oration in the Hebrew language or the recital of a chapter from the Bible. This I used to do with great gusto, to the delight of the parents of the initiate. The ceremony usually took place on a Sabbath afternoon at the home of the boy. Both the celebrant and I were showered with nuts, candies, sweets, and other presents. The pleasure that I experienced in bringing these home was greatly enhanced by the joy and radiance of my mother, who greeted me upon my return.

One particular event I shall never forget. I was barely eight at the time. My little sister was then only about a year old. I was carrying

her in my arms while running some errand for my mother. My path led me close to one of the four prayer houses of our town. As I was passing nonchalantly, I was grabbed in the back by two bearded men, one a local rabbi and the other a scribe who had just finished copying the Old Testament, or the Torah. I was carried bodily into the synagogue in spite of my violent protests. They made me leave little Miriam at the threshold, stood me up before a huge scroll, and told me to read a sentence of the handwritten Bible. It was an unwritten law that when a scribe finished copying the Torah, upon which he had spent at least three months of solid labor, every word had to be checked carefully by a rabbi. If only one letter in the whole script was misspelled, the parchment was pronounced unholy and could not be placed in the synagogue. Some of the letters in the alphabet, notably the fifth letter *heh* and the eighth letter *hes*, could easily be confused. The custom was to have an innocent child examine the script, and whatever his dictum was became the final decision. A situation of this nature arose now. To my great disgust, especially while I had to listen to the pitiful cries of my little sister at the threshold, I was requested to read the word in question. I complied with the request, by a correct reading, to the great joy of the scribe and rabbi. Little did these good people know that at that time I had memorized most of the Scriptures and needed only a glance to recognize the passage, which I proceeded at once to read from memory rather than from the written text. I was released at once. I grabbed little Miriam and ran for dear life.

Memories of Childhood and Youth

MEMORIES of childhood and youth! How they do impress themselves on one's mind! The home, the family, the friends, the town, and, not least, the countryside. If one could only describe the meadows and the brooks at the edge of the town! If one could only sing of the soft Ukrainian nights, especially the long twilights! If one could only hear again the singing of the nightingale! Or breathe again the dry air of those spring and summer evenings saturated with the fragrance of the flowering trees—the cherry, apple, pear, and plum trees—and the soft aroma of the melons and other garden vegetables, growing in such profusion! The smell of the open fields, early in spring, when being plowed, and late in fall when the crops were harvested! That odor of the black soil so filled my lungs that I was never able to forget it: it was later to lead me to the study of the natural processes that are responsible for this aroma.

I can best express my own feelings by letting the poet Gogol speak:

Have you any idea of the beauty of night in the Ukraine? You do not know the Ukrainian night! Just look at it: there is the moon in the center of the sky; the limitless horizons appear to have separated, to become even more limitless; the earth is all immersed in a kind of silver light; the wonderful air is cool and is full of desire and of aroma. That night is blessed by the Almighty! The wonderful night! The forests are bathed in peace, they are enthralled, and full of quietude, darkness and shadows. Peaceful and restful are the ponds; the cool, refreshing waters are encircled by the deep-green walls of the orchards. The virgin masses of wild cherry trees have spread their roots into the

coolness of the streams and shake their leaves occasionally, when disturbed by the cold night wind, which sneaks up on them and embraces them. The whole landscape is at rest. Above it all there is a breath which is marvelous, which is solemn. And your soul as well is at peace. Adorable night! Divine night! Suddenly everything comes to life: the forests, the ponds, the steppes. There sounds forth the magnificent thunder of the Ukrainian nightingale, and one feels as if the moon itself is listening to it in the center of its firmament. As if bewitched, the village on the hill is slumbering. The huts appear even whiter, they shine even brighter in the reflection of the moon, their low walls appear silvery out of the darkness of the night. The song has now abated. All is peace again.

The peasant lads and maidens returning in the twilight from their day of labor either in the sugar-beet fields or in an adjoining factory sing as they ride closely packed in the wagons. They spend the evening and late into the night dancing and singing in the meadows and in making love. How deep and soft are those songs of the Ukraine! They have hidden in them the suffering of a people, held down for centuries by the yoke of the oppressor. They possess the freshness and the breadth of the steppes, of their beloved Ukraine. I would listen to those songs deep into the night. I was carried away by them into the unknown. They made me dream of past ages and of future hopes.

I well remember the many holidays—those days of joy and sorrow. Those days when my father led me to the house of worship, where I was overawed by the recital of the history, the law, the moral teachings of the wise men of my own people, the people of the Book, devoted to learning, who derived greater joy from reading a brilliant exposition of the law by an ancient scholar than from a great feast or from other worldly pleasures. How much they have suffered, how this long suffering has expressed itself in the prayers on the Holy days before the Almighty!

Who can ever describe the Day of Atonement in a poor synagogue in a small town in the Ukraine! The day when all the men and all the women spent their time in fasting and in praying for forgiveness of the sins they had committed during the year. That sacred music sung by the Cantor and his choir penetrated into every fiber of my small body and filled me with hopes for better days to come. Who can

describe the happy festivals full of rejoicing, where one expressed the sentiments of his innermost self and identified himself with the Holy Writ, with the Holy Torah! Who can ever understand but one who has absorbed it through the pores of his whole body from his earliest childhood! Who can interpret the dancing and the merrymaking of the Hasidim,* those who believed in the joy of living, in the joy of attaining unity with God, and who found consolation in the Word of God.

I recall those days of prayer, with the men in the house of worship and the women of the neighborhood assembled in our home. They came to listen to my mother read and interpret to them the history of their people, a people who have given so much to the world, the Law, the principle of Justice, the very God himself. Perhaps I resembled in this respect another little boy who, in similar surroundings, must have listened nineteen centuries earlier to his own mother Miriam recite the past events of the same people. Both mothers, that of the first century and my own, nineteen centuries later, must have recited to their respective first-born sons the background of their people, that was ancient even at the beginning of the first century, such as their passage from Egypt, the building of the homeland, the destruction of the Temple, its rebuilding, and the constant hopes of a dispersed people to come back to its homeland, to live in the spirit of the Law of the Land. I listened to those laments and I understood. This was true especially of the ninth of the month Ov, when the destruction of the Temple took place, a day of sorrow and cause for special prayer.

Other memories, other events! Family celebrations, family gatherings! When as a mere child, only six or seven years old, I had to stand in my socks on a decorated table, bless another child who was to be initiated into school, then proceed to recite a chapter from the Holy Book, and interpret it in the light of the events of the day. I was in turn blessed by the learned men, by the rabbis and teachers, and finally by my own mother, who with shining eyes witnessed her child, her only one, growing up to be a pride to her and to her people.

Then those childhood friends, those many boys and fewer girls

* A religious Jewish sect that originated in Eastern Europe during the middle of the eighteenth century.

of the neighborhood, with whom I played and fought. Those gangs of boys from the wealthier, the more respectable, part of town, to which I belonged, and those made up of the children of the artisans, the poorer classes, who lived on the other side of the market place, in miserable huts. In my own group, I was not the biggest and certainly not the strongest, but I trudged along with the rest of the boys and did my share in helping to win a scrap, or if it had to be lost, in losing it like heroes, like the Maccabees of old.

Late afternoon on summer days, we would go bathing in the brook adjoining the town. Often, on the way back, we would climb cherry trees, apple trees, or pear trees, depending on the season, and steal fruit. The peasant women would rush out after us, but usually we were too swift for them. On one occasion, as I now recall to my great shame, as luck would have it, I was on the tree dropping the ripe, hand-picked fruit to my comrades below. When we were discovered, my friends managed to escape, but I was caught. The owner, who knew my mother, did not thrash me but kept my cap as a trophy. Imagine my disgrace to have to return home without a cap, which was at that time a requisite headgear for boys of my age, to the amusement of all the girls in the neighborhood.

Gradually, as I grew older, other games took the place of the wild fights in the market place. These were largely mental exercises, in which playing of chess and discussion of literary and other problems played an important role. Clubs were organized for common reading, for writing essays and short stories, first in Hebrew, then in Russian. Yiddish was usually looked down upon as the language of the common people, hardly worth consideration of intellectuals. None of my poetry or prose ever saw the printed page, but it kept me occupied in continuously creating and searching for new forms of expression.

Thus childhood gradually passed away, and I became a youth. As one who had grown up within a family of elders without other children, I was a bashful youth, who at the sight of a pretty girl would blush to my hairtips! When a girl approached me, I would shy away uneasily, especially when one was audacious enough to touch me.

I began to find more and more consolation in books, especially those that gave play to my imagination. First came *Robinson Crusoe*, the stories of Jules Verne, and *Life along the Mississippi* by Mayne

Reid. These were later replaced by Shakespeare, Ibsen, Hamsun, Tolstoy, and Dostoevski. I read history, biography, literature, travel. The last excited me most. After having finished several books by Fenimore Cooper, I definitely decided to proceed at once to America to fight the Indians. I dreamed of the Orinoco and the Amazon Rivers, where I was going to discover new tribes, new herbs, new remedies to cure human ills. Then came the Himalayas and other regions as yet undiscovered, which eventually I was going to conquer.

I see myself first as a boy of eight or ten with pink cheeks, black curly hair, shiny eyes; I was usually dressed in a long coat, the costume of the day. As I grew older, the coat became shorter. My regular Sabbath attendance of the religious services became less regular. As my interest in languages and cognate subjects became greater, my interest in the biblical and talmudic subjects diminished.

As a rule, I was rather healthy. Of the various children's diseases, I remember only scarlet fever. For days, my mother kept constant vigil over me. I remember her sitting on a bench near my bed, reading by the light of a kerosene lamp from some book. When the crisis finally came late one night, I remember she uttered a sigh of relief, covered me tight, and lay down near me to rest, probably for the first time after many sleepless nights. Later, I seemed to have suffered for a few months from a weak heart; this involved drinking excessive portions of fish oil and not attending regular classes. This, too, passed off without permanent damage.

The extensive reading that I did taught me a lot, but these studies were not systematized and were poorly organized. They hardly prepared me for anything that might be called a future career. At best, I was on the way to becoming a dilettante, with a cursory knowledge of many things, but without a fundamental background in any.

As I reached maturity, however, there occurred a great catastrophe in the land, which changed profoundly my whole future life. The Russo-Japanese War of 1904-1905 ended in a great calamity for the country. It served to awaken the people to their miserable position, largely as a result of the rottenness of the system of government and of a type of society that was satisfied to retain its ancient mold which benefited the few at the sacrifice of the many. The people demanded a change. They requested political freedom, the right to participate

in their government. They held numerous meetings and discussions; they condemned the government; they built barricades; they organized general strikes. The whole country was inflamed. The railroads and the ships, the factories and the shops, all came to a standstill.

Finally, the Czar was forced to declare that the people would be granted certain privileges, and that a Parliament, or Duma, would be established to take an important part in the affairs of state. But no sooner was such an announcement made than the Cossacks and the police swooped down upon the towns and villages, where innocent people were subjected to death and destruction. All the leaders of the movement against the government were arrested, some were shot, and others were dispatched to far Siberia. All organizations were ruthlessly repressed. Gatherings, even of small groups, were prohibited. Political discussions were considered crimes. The freedom gained in a sea of blood was soon lost.

Although our town was too small and too far from the big centers to indulge in revolutionary activities, the local youth would gather clandestinely to discuss potential political changes and analyze in detail the doctrines of the various socialistic movements that were sweeping the country. These discussions were never followed by any direct action, either against the state or against any industrial organization. The local policeman could sleep quite peacefully and continue to collect his bribes from local merchants for overlooking their infringements of petty laws. I have already mentioned the fact that my own home was raided and all my books carried off for careful inspection. When it was discovered that the books contained nothing more dangerous than literature and history, I was promptly released. My family was well known in town, and the policeman probably made the raid as a matter of routine. Possibly my travels to the neighboring cities to find suitable teachers may have given rise to suspicion that I was in the process of organizing an illegal group.

Although I was still too young and lived too far from the great centers to participate in any political demonstrations or activities against the oppressor, I was old enough to understand the deep waves of dissatisfaction sweeping the country. It was now time to stop and ask myself: "Where shall I go now?" "What shall I do next?" Certainly, my old form of life could not continue much longer in my

home town. I was in possession now of all the knowledge that my town was in a position to offer me. It was time for me to leave the town and go out into the world. But where, how, and for what purpose?

My mother watched me with anxious eyes. She had been dreaming and hoping for some time that before long I might find a suitable girl whom I would marry and with whom I would bring up a family. But she now saw, with bitter feelings, that this was not to be the fate of her child, her only son. She recognized that she would have to sacrifice her hopes and desires, so that her son could attain his aims, his ambitions, his purpose in life. But what were these to be? She did not know that any more than did I. There was no question, however, that the time had arrived for me to go to some big city, where I could obtain more preparation and learning, perhaps even enter a university.

The breakaway did not come at once, but gradually. It did not prove to be as difficult as I expected. I was helped and encouraged in my efforts by my mother, by my teachers, and by my comrades, who looked to me to open a way for them to follow. We proceeded at once to search for a well-qualified teacher who could train us in subjects required for passing Gymnasium examinations. We finally found one, who himself was being prepared for entering the university.

A year of intensive study thus passed rapidly. I now felt that I was well prepared to submit myself to an official examination to test the proficiency of my knowledge. This had to be done in a city which had a government Latin school or Gymnasium. I selected the city of Zhitomir, which was the capital of the neighboring province of Volhynia. This choice was governed both by its proximity to our town and the fact that our teacher came from there and was, therefore, acquainted with the particular requirements of that school. To qualify for entrance to a university, I would have to complete a full eight-year program. I was not prepared for this, but I could attempt to take the examinations for the first five years or classes. Having passed these, I could then go on with the rest of my program.

In the spring of 1907, I proceeded, some six or eight weeks before the examinations, to Zhitomir. There I rented a room, engaged special teachers for a final check-up of my preparation, and applied for admission to formal examinations. These were to be held at the end of the school year, in May, and were to last about two weeks, each day

a different subject. I was very brave when I started with the examinations, passing successfully one after another, but finally I failed.

Even as I look back now on those examinations, more than four decades later, I feel faint at heart. Not that I did not possess as much knowledge in the required subjects as the regular students of the Gymnasium did, but my knowledge was not well organized, certainly not in the manner officially required. I could not beat the system. It was a hard, cruel system, which made easy prey of a boy who tried to beat it; a boy who had come from a little Godforsaken town. How did I ever think that I knew as much as those neatly polished and uniformed young fellows who had been attending the same school regularly for years! I must have appeared in their eyes a provincial yokel, a simpleton who did not know what he was up against.

But I was persistent. I had a legal right to require to be examined, and I was examined. First came the written examinations in Russian literature and in foreign languages. These I passed well. Then followed the orals, which came off, if not brilliantly, at least satisfactorily.

Finally, on the last day, I was to be examined in my beloved subject—geography. I felt more sure of passing that examination than any other. Did I not know every country in the world, every important city, mountain, and river; had I not, in my mind, traveled through all the countries, climbed all the mountains, paddled all the rivers! Did I not dream of visiting those faraway places in the future, of traversing them by foot and by rail! But I was in for a great disappointment. I now came face to face with the bureaucratic system, and I was licked.

The teacher who, dressed in official uniform, questioned me must have been tired out after a year's heavy work; he was no doubt hoping to leave soon the dreary and dusty provincial Ukrainian city for his vacation abroad or at least in a country home; he was sleepy, no doubt having spent a sleepless night arguing with his wife whether to spend the summer vacation in Switzerland or in the Crimea; he may have been worried over the limited financial returns that, as a teacher, he was receiving. He looked upon me, a rather cheerful youth from a small town, of which he no doubt had never heard. He must have made up his mind at once that it was not worth even the time that he was to spend on this youth, time that could be spent far better in

playing a few more minutes of tennis or helping his wife with the packing of the trunks.

He looked at my card, then at me, with his humorless, half-closed eyes, as if to say: "So you are determined to pass these examinations? Well, it appears that you have actually passed most of them! Not so fast, young man, not so fast! You are not through with it yet!"

I answered his first question satisfactorily, but his next one, about the river that flows through the city of Berlin, puzzled me. I knew the names of every river in Germany: I had dreamed many a time of future travel on a boat along the majestic Rhine, with its castles and its historical ruins; I knew of the broad Elbe, the slow-flowing Oder, the Moselle, the Main, and the Danube; but I could not think of the one that passed through Berlin.

The teacher was adamant: there was a river, and I must know it. I began to argue that it was hardly an important river, that at best it was quite insignificant, as compared to the other rivers that I mentioned. All was to no avail. The teacher demanded an answer. He had me where he wanted me, and he would not let me out of the dilemma.

In vain did I beg to be asked other, perhaps even more difficult, questions. He would not even listen further. He closed his examination book, wrote down his mark, which was a zero or one close to it, opposite my record. Of what importance was it to waste more time on this provincial lad, who no doubt would never amount to anything anyway? If this lad did not fail now, he would be failed by some other teacher. He might as well save himself further trouble and go back home to the little town where he belonged.

A single zero, in only one subject, canceled out all the other examinations, no matter how well passed. Another year would now have to roll by before I could present myself for another examination, and I would have to go through all the old subjects, in addition to the new ones.

When many years later I visited Berlin, now as a young scientist, coming to meet colleagues in my selected field in various German universities, I made a pilgrimage to the river Spree. As I stood upon one of its small bridges, I mused on how the mere name of a river, scarcely known to those outside the city through which it flowed, could influence the whole future of at least one human being. Had I passed

my examinations in 1907, perhaps my whole life might have been different! "What's in a name?" Well, sometimes, as in this particular case, a lot of human happiness may be bound up with a name.

Crestfallen, I returned to my native town. I no longer wanted to stay in this city of Zhitomir, to which only two months before I had come with such high hopes, with so many expectations. At home I was met by my comrades, who were looking forward with much excitement to having at least one from their midst break away and open a window into the great world. They were more discouraged than I. If I had failed, what chance did they have! True, two or three of the older boys and girls of the town had left before me, and some had returned with diplomas in pharmacy. One or two had entered the much-coveted Gymnasiums. One had even entered a university, the highest attainment of one's ambition. But most of them had emigrated to America, to the New World. Those who had come through with diplomas were the children of the rich, the few in town who could buy their way to education; very few of the middle and poorer classes had ever accomplished that. It was the poor who emigrated.

What was I to do now? Was I to try again? Was I to battle my way, to force a path for myself to the university, to that dreamland, to that fountainhead of all knowledge? What should be the next step? My comrades asked that question openly. I had only to point the way, and they would follow.

My mother asked no questions. She knew that I would try and try again. She only looked at me. Her eyes were full of disappointment but they also spoke of courage: Keep trying, my son, do not give up so easily! This was ample proof to a youth that life is a hard struggle. There could, therefore, be only one answer: I must prepare further, I must find other and better teachers, I must take another step and present myself for examination before another Gymnasium, perhaps for the next, higher class.

I continued my studies both at home in Priluka and in Vinnitsa, my father's home city, where I lived with one of our tenants. I found a very warm atmosphere among the many youths in that city who, like myself, were striving to break away from provincial life, who were groping for a new world and for new ideas. In their company, I found broader horizons than in my small town. I also found better teachers.

Unfortunately, this city of nearly forty thousand inhabitants had no Latin school, and hence could not offer me the opportunity to take the official examinations. I finally decided to continue my studies in one of the largest cities of our region. There I could get the proper preparation under well-qualified teachers. It is there that I would try my fate again. There were only two such cities in our region, Kiev and Odessa. The choice fell upon the latter.

The limited funds that I had earned by tutoring during the previous year, supplemented by some financial assistance from my mother, were just about sufficient to enable me to hire good teachers and devote all my time to the required work. I plunged into this with all my heart and soul. I worked hard, as hard as I had ever done before. There was a goal to be attained, and it would be attained no matter what difficulties were to be found in the path.

In undertaking a difficult program of study, I had to depend a good deal upon my memory. It was a good memory. But it was visual, and not auditory. When I read a book, I could almost remember every word, but I did so as if each page were held up before my eyes. I could see before me the words that I read, but I could not remember the words that I heard. Thus, even in subsequent years, when I listened to numerous lectures in and out of classrooms, I had to write down every word. I might never refer to the notebook again, but I could see those pages fluttering before my eyes as I tried to remember the contents of those lectures. As a youth, when most of my learning depended entirely upon reading books, and even in the case of certain practical exercises, I had to read rather than to listen. This gave me a marked advantage over my classmates, since, having read a book once, I could easily remember the contents. I was, however, at a distinct disadvantage when it came to music and other forms of art and learning where my ear had to function. Thus, I never learned to appreciate musical compositions, except the lighter forms, the sounds of which would evoke images of movements of people, of laughter, of dancing.

Recently I read that Justus von Liebig, the great German chemist, also completely lacked an auditory memory. He said: "Nothing or very little of what one learns through this sense [hearing] remained with me; I found myself in an unpleasant position as a child." He

had, however, a perfect visual memory: "Everything that I saw, on purpose or not, remained as a photographic reproduction in my memory."

I was thus deprived of the pleasure that comes from listening to a brilliant speaker or of enjoying a great musical composition. If a lecture was important, I could write it down and remember it. Unfortunately, I could not write down the music. Possibly if I had been taught harmonics in childhood, and if desks were provided at concert halls, I could also derive pleasure from the great compositions that thrill so many and that, in my case, fall truly upon deaf ears.

In my scientific work, and in the period of training for it, I had to perform experiments with my own hands, or watch them being performed by my students and assistants, to appreciate their full significance. I was often embarrassed when questioned about a previously discussed subject and had to request repetition of the conversation, so that I could fix the picture in my mind by special concentration. This is, perhaps, the reason why I liked mathematics, why laboratory experiments appealed so much to me, and why I had little interest in mental disciplines. I remember especially an incident at the end of my freshman course in general chemistry in college. The professor handed me a white salt and said: "See whether you can tell me what it is." I could not analyze it for any of the elements, since I had not yet studied qualitative analysis, and the professor would have objected to that approach. The only tools I knew how to use were the analytical balances and the Bunsen burner. So I carefully weighed the sample before and after the loss of water of crystallization and calculated from the results that the salt could be only gypsum or hydrated calcium sulfate. The good professor then addressed the class somewhat as follows: "Those of you boys who had chemistry in high school are at a great disadvantage when you take chemistry in college. You think you know it, when you don't. So you don't pay attention. Perhaps, those of you who are preparing to be chemists should avoid the subject until you come to college. And do pay a little more attention to your mathematics." He thus came to the same conclusion as the famous German chemist Eilhardt Mitscherlich, who stated in his introduction to his textbook: "Nobody will contribute much to chemistry who occupied himself with it before he reached his sixteenth year."

In Search of a Future

TO FAMILIARIZE the reader with the peculiar nature of the type of education that I was pursuing, I must describe in greater detail the existence in Russia in that period of a peculiar type of student known as an *extern*. This was a nonmatriculated, or extra, student who was educated by private tutors and who presented himself at the end of the academic year before one of the government middle schools, usually the Latin Gymnasiums, to take the formal examinations. These examinations were similar to those taken by the regular students in school. Upon payment of a suitable fee, all those who applied were examined in all the subjects they were expected to know. If the applicants passed these examinations satisfactorily, they were given diplomas indicating that they had completed so many years of education, or a course of training equivalent to the corresponding number of years in the school. This entitled the externs to all the rights and privileges of the regular students.

The extern had the advantage that he could skip as many classes as his ability and training allowed; he could come up for the examinations when he felt prepared for them; and he could select any Gymnasium in the country at which to present himself for such examinations. In my own case, having failed to pass the examinations for five classes of Gymnasium in Zhitomir, I could still present myself in Odessa, two years later, for the examinations of seven classes, and finally for eight classes, or final "maturity examinations," in 1910.

There were certain serious disadvantages to being an extern. The teachers did not come in daily contact with the applicant. They did

not know him and thus tended to be more severe in their questioning of him and in their attitude toward him. Failure in one subject meant failure of all examinations taken at a particular time. The regular students could benefit from the credits given them for their daily work, whereas the externs had to depend for their grades entirely upon the final examinations. There was another disadvantage in being an extern: one could never obtain the coveted gold medal or even very high grades, which would facilitate entrance into the university.

There were three types of students who took advantage of the extern system. The first were the sons and daughters of the very wealthy, and even of the nobility, who did not care to attend regular classes and thus be subjected to the daily discipline of grinding studies. This is well described by Leo Tolstoy in his book *Childhood, Boyhood and Youth*. The second group comprised the poor and underprivileged, who had failed for one reason or another to take advantage in their early youth of a proper education and who tried to catch up later in life. The third group comprised boys and girls from the small towns, especially those of Jewish descent, who, because they lived far away from a school center or because of the existence of a slim quota, had little opportunity to enter the few Gymnasiums in the region.

Born, as I was, in a small town where there was no Gymnasium, of parents who thought more in terms of a religious than of a secular education for their only son, belonging to a race systematically prevented by the government from receiving an official education, I was forced to take advantage of the limited opportunities that were left. I was, therefore, forced to become an extern.

As a whole, our town passed the revolutionary period of 1904-1905 with the subsequent reverberations without any ill effects to its youth or to the population in general. No strikes occurred, no pogroms, no burning of estates by peasants; the mutual relations between the various racial and social groups remained rather peaceful. Occasionally, a revolutionary agitator would arrive in our town but, after suitable consultations, he was promptly dispatched, with the conviction that our town hardly offered a basis for revolutionary activity. We would assure him, however, that when the time came and the need arose, we would probably manage by ourselves what little would have to be done.

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There was another problem, much more serious than the revolution itself, which threw fear into our town. One of the most despicable tricks of the old Russian government was to use the Jews as cat's-paws for diverting from it the anger of the city and village population. When it appeared that violence was about to break out against government agencies, either among the workers or among the peasants, intensive propaganda was started at once through the government-subsidized press, or through some other devious and insidious channels, to the effect that the Jews were all socialists and revolutionists and were largely responsible for the major hardships that had befallen the country. This detracted the attention of the fighters for freedom and of the intellectual classes from their attacks upon the vicious autocracy of the land toward the protection of the innocent minority. This type of sinister propaganda has served many a group of rascals in other times and under other conditions to save their own skins or to promote propaganda of a special kind.

In our town, as in others like it, there was always the possibility that many of the half-ignorant peasants would fall for such propaganda and would thus be brought to a state where their hatred of the government and of the landlords would be diverted toward the harmless and unprotected Jewish communities living in their midst. Although we, in the towns, knew full well that sooner or later the time would come when the whole system of government would collapse, that it was bound to crumble at the first serious impact from outside forces—as it nearly did, in 1905, following the Japanese War, and as it finally did during the first World War in 1917—nevertheless, for the time being the potential danger to the inhabitants of the small towns existed.

Knowing of such possibilities, the youth of the town were forced to engage in the preparation of a program of self-defense. We were fully aware that in case of danger the police would either stand aside, on orders from above, or absent themselves altogether and would allow the peasantry to arrange pogroms on the Jews, as had happened in many cities and towns in 1882, in 1905, and at other periods. Each one of us who was old enough to handle a weapon had learned the use of revolvers or of other fighting equipment in case of emergency. Fortunately, that never came to pass in our town, since it was too small

to be of much significance in the general scheme of things, and especially since the relations between the townspeople and the village people were both friendly and collaborative.

I must dwell here upon two aspects of my early education that I have not yet touched upon, those of sex and politics. As an only child, brought up by my mother, grandmother, and numerous aunts and cousins, I naturally felt strange in the company of girls of my own age. To be sure, there were several girl cousins around, but our relations were of simple family associations. Occasionally one of these would become definitely practical, looking upon me as a potential future husband; this had but little appeal to me. All my friends were young men of my own age. Some of the older boys with whom we often associated would tell in a spirit of bravado of their relations with peasant girls, but this would tempt few of us.

We were so much engrossed in the tremendous tornado of the political scene, the important social changes that were passing before our very eyes, and the urgent problems of education facing us—that we hardly had time for considering the feminine members of the community in terms of sex. The few girls who formed a part of our own group or were close to its fringes, like the sisters of some of my friends, were considered as comrades. Thus, Masha, a girl with strivings similar to our own, was also shaping her life in terms of current problems facing the world rather than of boys as such. True, most of the members of my close circle of friends fell periodically in love, one after another, with Masha, and even had ambitions of becoming her future husband. These were passing fancies that nobody took seriously, nor did they interfere with our work, plans, or philosophy. The other girls may have had designs on some or all of us, but these, as well, resulted in temporary flirtations and led to no serious consequences.

I thus grew up in my town quite innocent as regards feminine society and potential future feminine affiliations. This caused considerable disappointment to my mother, who was waiting for the day when I might select one of the pretty maidens in the town with whom to settle down in wedlock and bring a family into the world. Regretfully, my mother died without experiencing what would have been to her the greatest of joys, that of fondling the progeny of her only son, her Zolman or Zolminiu, as she would call me.

My political outlook of that period also deserves mention. The young people of the town, at least those interested in the future of the country and their people, could be divided into two major categories. First, there were the Zionists, or those who were hoping that Palestine would become the homeland of the Jews. These included the Hebraic scholars, the children of the wealthy, and the dreamers who progressed from the study of the Bible to that of more modern Hebrew literature. Second, there were the socialists and the revolutionists who were looking to the salvation of their people in Russia itself and who were hoping that a basic change in the government would make Russia an ideal place for a future life. These were mostly the children of the poor, the artisans and such Talmudists as had no stomach for a purely Jewish Eden.

I found myself on the fringe of both groups, with divided sympathies. I was already becoming interested in the whys and wherefores of nature and could be but little attracted to the interests of either group. By training and tradition I should have joined the first group, but I could not agree with their narrow nationalism, their limited outlook on life and its problems. By nature and inclination, I logically belonged to the second group, since I was hoping for a change in a government that was choking the life of all the peoples inhabiting the great Russian empire. I could not stomach, however, the existence of the many revolutionary groups who could not agree among themselves upon the future form of government should a revolution come about.

Life in a small town may also have had something to do with this political outlook. Had I lived in a great city, had I come under the influence of great thinkers and orators, I might have been swayed in one or another direction. Further, I was already beginning to show interest in scientific principles, which could hardly permit me to swallow unquestioningly some of the political philosophies with their broad generalities and frequent appeals to passion rather than to intellect. It is because of this that both the philosophy of Ahad Haam, the Zionist thinker and idealist, and that of Belinski and Chernyshevski, the revolutionary Russian liberals, left me cold, although I greatly enjoyed reading their works. It was the philosophical realists, the writers like Ibsen, Hamsun, Thackeray, Dickens, Zola, Tolstoy (his novels

only), Ostrovsky, Andreev and Artsybashev, to mention but a few, who made me think and analyze the realities of life.

The city of Vinnitsa, where I spent part of 1907-1908, possessed a distinct intellectual atmosphere, with a number of schools which were on a par with Gymnasiums, although graduates of such schools could be admitted to universities only upon passing special competitive examinations. There were in the city a fine library, a theater, and a number of qualified teachers who could prepare me for my examinations.

In taking up residence in Vinnitsa, I was close to my home town; I could spend week ends with my parents and see my friends. My mother would often come to visit me. She would be loaded with parcels of food and other provisions.

I enjoyed particularly the weekly trips home, which took place on Thursday. We would leave the city at twilight. The trip would take us three or four hours in a wagon or sleigh drawn by one or two horses through various villages and over several rivers and brooks. The wagon or sleigh might carry passengers returning home to spend the Sabbath with their families. Often it would carry goods ordered by local merchants.

I recall particularly one of those drives. It was on a bright starry night early in April. The snow covered the ground and the still-frozen rivers. The small sled was packed with dry goods ordered by one of the town's merchants and pulled by a sprightly horse. The coachman, like the horse, was a spirited individual, who greatly enjoyed his work. I was the only passenger. We left Vinnitsa about nine in the evening and expected to be in Priluka about midnight.

We left the city in a gay mood, tuned to the peaceful earth, covered by the bright, freshly fallen snow, the well-illuminated sky. The horse knew the road so well that neither the coachman nor I was much disturbed by the road conditions. I recall now, forty-five years later, the bright moonlight and the limitless snow-covered fields, which glistened in the moonlight. Here and there we passed a sleeping village, with its snow-covered, straw-thatched adobe houses. Most of the lights in the houses were already out, although here and there a small kerosene lamp gave out a dim light through a tiny window.

It must have been about eleven o'clock when we approached the

river Bug, which flowed through our neighborhood. We were only six or eight miles from Priluka. A fairly steep hill led toward the frozen river. Both the driver and his passenger were in high spirits due to the effects of the moon, the snow, the rapidly moving sled, the frisky horse, the approaching warm home where my mother was awaiting me. We paid no attention to certain bunches of twigs attached to the telephone poles along the road. These signified, in the language of the peasants, that the ice in the river had begun to melt and that crossing the river on the ice was not quite safe. I jumped off the sled and ran after it, keeping up with its speed, although at some distance behind it. The sled was just about in the middle of the river and I was approaching its shore, when the ice broke. Down into the icy waters went the sled with the driver and the horse. By some miracle, the horse managed to keep its head above water and the driver held onto the horse for dear life.

I turned at once and ran back to the village on top of the hill. All the houses were now dark. I stopped in the middle of the solitary street and began to shout as loud as my lungs would permit, "*Liudi dobre, spassuite*" (good people, save us). Half-clad peasants, men and women, carrying poles and ropes, came pouring from all the houses. They rushed to the river and succeeded in pulling out the sled with the horse and the driver, unhurt but nearly frozen to death.

The peasants were more concerned with the health of the horse than of the driver. A half-dozen men piled into the sled and began to drive the horse as fast as they could to make it sweat thoroughly. They decided to take us to the local prison to lock us up for the night, to await the decision of the judge in the morning. We were accused quite rightly of being disturbers of the peace, since we had not observed the markings on the road. The driver was not to be outdone, however. He suggested that we stop, on the way to the prison, at the local inn, and have a good drink of vodka. This suggestion was gladly accepted. The innkeeper, an old friend of the driver's, brought out all the vodka he had in his possession. This pleased our saviors immensely, who were happy to enjoy the drink while we went into the inn. Having consumed a bottle or two each, our peasant friends did not care much where we spent the rest of the night and returned to their own homes in a jovial mood.

The innkeeper and his wife prepared some hot food for us and placed us on the top of the warm stove, so that we could rest after the exciting event. We left the friendly inn early in the morning, found a suitable bridge for crossing the river, and arrived in Priluka an hour or so later. The exciting events of the night dampened our feelings only temporarily, since we approached our native town none the worse for what had happened. My mother was in the process of baking fresh bread for the Sabbath. Hot bread, just out of the clay stove, and thickly covered with a layer of fresh butter—no bread ever tasted better to a weary traveler!

As soon as the cherry blossoms began to drop and the apple blooms appeared, I returned home for the summer and resumed my tutoring. I had already decided that when fall came, about September (1908), I would go to Odessa, where good teachers were to be found, where my studies could be pursued more systematically, and where there were several Gymnasiums to which I could present myself for the examination. Peisi and two other friends were to go with me.

That fall four provincial youths, ranging in age from eighteen to twenty, with healthy pink cheeks barely covered with a thin bloom of hair, arrived at the seaport city of Odessa, some three hundred or four hundred miles away from their native town. Loaded with parcels of food and clothing and boxes of books, we traveled nearly a day and a night to reach our destination. Sprawled on an upper shelf of a fourth-class carriage, packed in with innumerable peasants, bundles, and other youths going to the same city, in similar hunger for learning, we cared little about the inconvenience of travel. We knew, of course, that the merchants and wealthier people traveled third class, the very wealthy people went in second-class compartments, and the nobility, whom one never saw but about whom one could only dream, traveled in the first class. But in our carriage we were happy, excited, and enjoying life.

At each station, one of us would rush to the dining room or to the boiler outside the building to fill our kettle with boiling water, so we could make fresh tea. After we ate our food and drank several glasses of scorching tea, we proceeded to sing in husky voices various revolutionary songs. We were soon joined by other groups, and the whole car became a concert stage in motion. The peasants looked upon us

with varied emotions, some approving, others disapproving, but most of them indifferent.

Odessa was a famous seaport city in old Russia. Its sprawling boulevards, its shiny quays, its port with the many boats coming from all over the world, its numerous theaters and concert halls, including a magnificent opera house, its beaches, its curative mud baths, its slums—all tended to endow this city with a glamour of which I had never before dreamed. A new world and new horizons opened before me. The fine university, the five Gymnasiums, the numerous other institutions of learning, the great cultural centers, gave a new zest to my present life and filled me with great hope for the future. The free and open life of the port city and its heterogeneous population comprising numerous nationalities of Russian and Mediterranean origin, served further to create a cosmopolitan atmosphere totally different from that to which I was accustomed.

Rather than engage a single teacher, as I had done before, I joined an evening school, where some of the teachers of the fifth Gymnasium were holding classes. The fees were rather high, but the instruction was excellent. I was sure that I would receive there the needed training in the required subjects. I would also become acquainted with the methods of those teachers who were to examine me later.

Thus I went to classes at night and did all my studying in the daytime. I soon adjusted myself to this form of life and took considerable interest both in my work and in the many attractions that the city had to offer. The teachers made their classes interesting, and although it was all book and paper work, with few laboratory demonstrations, I was fascinated by it all. I made frequent trips to libraries and museums, and, when funds would permit, to theaters and operas. Our meals were modest. We arranged with our landlady to prepare some of them for us. This gave us a chance to go out often to some of the cheaper restaurants in the city.

During the week ends, we visited some of the political gatherings in various assembly rooms and in private homes. None of the political ideologies impressed or attracted me. I was more interested in my work. We would often gather in the rooms of some newly acquired friends and discuss our work and the forthcoming examinations. Girls, as such, had little attraction for us at that time. The few that we met

at the homes of friends were not singled out for special attention.

Among my new teachers, two had a particular influence upon my mode of thinking.

One was Kingi, who taught composition and Russian literature. He was a giant, with a huge head, a heavy beard, and a mane of blond hair. He had been trained at the University of Moscow under some of the leading literary lights and was enthusiastic about his subject. He found our group of small-town boys far more interesting than his regular classes at the Gymnasium. He instructed us not only in the art but also in the science of composition. A devotee of Turgenev, Tolstoy, Dostoevski, Chekhov, Ostrovski, he tried to develop in us a proper appreciation of these Russian literary masters. In this, he was eminently successful, and I always looked forward to his class.

The other teacher was Tarnarieder, a hunchback, who taught us mathematics, physics, and chemistry. A native of Odessa, he had been trained at the Sorbonne in Paris. It was he who organized the evening school and served as its director, since he could not find a position either in a university or in a Gymnasium because he was a Jew. Although he had no laboratory and had to do all his teaching by the use of a blackboard, he was highly enthusiastic about his subjects and succeeded in imparting to us his devotion to science. Long after the classes were over, he would remain discussing with a small group of us the organization of European universities, their methods and systems of education. America was so far away that its universities were seldom mentioned.

Some of my other teachers, notably one who taught European and ancient history, were also graduates of universities in Western Europe and could thus bring to us first-hand appreciation of European history and culture. Of the greatest importance to us, however, was the fact that some of these teachers, one of whom was always drunk in class, were instructors in the fifth Gymnasium and were thus able to prepare us for the type of examination that we were to face at the end of the year.

The fall and winter of 1908-1909 passed rapidly. The warm winds, harbingers of spring, began to blow from the south over the Black Sea. We were busily engaged in preparing for our examinations. I well remember the May fifteenth when I submitted my appli-

cation for examination for classes. The office of the director of the Gymnasium was filled with hundreds of young men from all parts of the Ukraine who had come for a similar purpose. We were notified that the applications of some fifty of us, ranging in ages from eighteen to fifty, had been accepted, and that we were to report on certain days for the various examinations. All the examinations lasted about two weeks, first written, then oral. Only those who passed the first group were admitted to the second. I came through with flying colors. Only four others passed. My joy was dampened only by the fact that Peisi, my childhood friend who had accompanied me to Odessa and who submitted himself for only four classes, failed utterly. I tried to console him and offered to help him during the coming year.

I returned home a hero. My mother was jubilant. My friends were happy. If I could do it, others would follow. I was the first swallow that promised the coming of the spring for the youth of my age in our town. There was one more hurdle to be overcome, and that was the final examination for eight classes.

I now prepared for another summer in Priluka. I would teach again and save some more money. Now I could command rather decent fees, since I could instruct others in the various arts and sciences required for official examinations.

One of my new students, the twenty-year-old son of a wealthy peasant, was anxious to avoid military service and could succeed only if he were in an official government school. The Gymnasium was out of the question, since his qualifications were too low. Possibly another type of school, such as a commerce school, the scholastic requirements of which were considerably lower than those of a Gymnasium, might accept him. At his age, he should have entered the seventh class, but his training scarcely qualified him for even the third class. The fact that he was a peasant and had money to pay made it possible for him, with a little coaching on my part, to enter the fifth class. Like many another school where Jewish parents were obliged to pay all expenses for a non-Jewish boy to enable their own son to receive a suitable education, this particular school admitted one Jewish boy for every non-Jew. I had to nurse my student for several months before he was prepared for the entrance examinations. But he managed to avoid military service, or at least postpone it until he was graduated from school.

That summer, I was smitten by the greatest misfortune that could have befallen me—my mother died. She was sick hardly at all. One day, while I was home, she developed an acute constipation. The local doctor was absent. I rushed her by wagon to Vinnitsa. A doctor was called in. He advised an immediate operation, which could be performed only in a first-class hospital. I had to take her by train to Kiev. This involved an overnight trip, which was very hard on her. Apparently an operation was too late, and the doctors tried to treat her by means of drugs. These proved of little help. I spent two weeks, day and night, at her side in that hospital. My father came once; some cousins came. But I was always there. Her last words to me were not of herself but of me: "My son, now that you need me most, I am going to leave you. Who will help you in your need!" That was the end!

I sat there at the edge of her bed and cried quietly. A kindly woman visiting a patient in a neighboring room came in, placed my head on her breast and tried to console me. Of what good was it! My mother was gone! I was alone! That night I spent in the morgue with the body of my mother. I rendered no prayers before the burning candles. I just sat there thinking, thinking of the past and of the future.

The next morning my mother was buried in the Kiev cemetery. A year later, just as I was about to leave Russia, hoping never to return to it, I visited Kiev and erected a monument on my mother's grave.

When I returned to Priluka, after having buried my mother, the whole town came to meet me. Shocked by her untimely death, everyone went into mourning. She was beloved and respected by all. My friends gathered around me. My father came up to me, his face covered with tears, and, with shaking hands, cut the lapel from my coat, in sign of mourning. As the Holy Book says: "And you shall tear the dress off your body, and you shall pour ashes over thy head, and you shall mourn the loss of thy beloved."

During the seven days of mourning, as I sat on the floor of our house, I reread the Bible, perhaps for the last time. I read carefully the passages of Job and Koheleth (Ecclesiastes). My friends remained faithful to me. They were always with me, trying to console and distract me.

My father wanted me to remain in Priluka or settle in Vinnitsa,

perhaps find a girl, marry her, and take over all his properties. This did not appeal to me. I had other things in mind. I was anxious to leave, to run away from it all. I signed over to my father the house in Priluka which had been built and owned by my mother. All I took for myself were sufficient funds to help carry me through the final year of my education.

Early that fall of 1909, accompanied by my constant friend and companion Peisi, we directed our paths again toward Odessa. I had to make a gigantic effort that this year should be my last one preparatory to a university career. I had seen too many "eternal students," boys and young men who were forever preparing for and never taking, or taking and failing, the examinations in Odessa. I did not want to become one of them. They spent most of their time in philosophical or political theorizing, without ever hoping to reach an end to their career of preparation. They dutifully presented themselves every spring at a Gymnasium for their annual examinations, but they seldom expected to pass and face another form of hardship, that of entering a university.

I knew from my last year's experience that I could, with a little supplementary study, pass the final examinations. I was well versed in Latin, German, and French. I knew my mathematics, my history, and other subjects, such as ethics and psychology. My greatest difficulty, however, was the Russian language and literature. I thought of our last year's teacher, Kingi. Soon after my arrival in Odessa, the five of us who had passed last year's examinations joined forces and engaged him to give us private lessons two or three nights a week. He agreed at a very stiff price, but he did a marvelous job in teaching us. He instilled in us a love for Russian literature and a thorough appreciation of the rules of the language and of composition in general.

Here my good memory helped me a great deal. Just as I had come to know by heart some of the Latin poets, just as I had known by heart the Bible in the original Hebrew since childhood, so I memorized easily these Russian poets, ranging from the clumsy lines of Derzhavin or Lomonosov to the graceful poems of Pushkin or Lermontov. Particular attention was paid to Ostrovski, Chekhov, and some of the other dramatists, and to Dostoevski, Tolstoy, and some of the other novelists. I assimilated their works, not as a student anxious to pass an

examination, but as one who had become permeated with their beauty. It was to me a labor of love. I would sit up late at night in my room absorbing the beautiful lyrics of Pushkin or the profound psychological discussions of Dostoevski.

When I presented myself the following May for the written examinations, the thesis assigned to us was the subject: "The insulted and repressed types in Russian literature." I knew this topic well and wrote, in the five hours allowed, a satisfactory essay, as certified later by the examiners. When, eighteen months later, this time in an American college, I was asked by my English professor to choose a topic for one or more required quarterly essays, I selected the same topic, and, during my four years in college, wrote sixteen consecutive essays covering this subject.

The winter of 1909-1910 passed rapidly, with only minor variations. With my mother gone, I no longer had a home to return to. I tried to forget the past by concentrating on my studies and supplementary readings. There was little recreation, except occasional visits with friends and long walks in the city parks and along the boulevards.

I had but few letters from my father. He married again, a little too soon perhaps, but I could understand; he was lonely, he needed someone to make a home for him. But I could not go back to my old home and see another woman installed in my mother's quarters.

Meanwhile, the date for the final examinations was rapidly approaching. There were plenty of problems ahead of me. First came an official government announcement that only those who had already received certificates for six or seven classes would be admitted. Since I had my certificate for seven classes, I was in line for admission. Later, it was announced that only those who had been born in Odessa or who had spent at least twenty years of their life in that city would be admitted. Here the corrupt bureaucratic system came to my assistance; with the help of ten rubles and two willing witnesses, I obtained without difficulty a certificate that I had spent most of my life in the seaport city. Out of more than two hundred applicants, only forty fulfilled all the requirements and were admitted.

The actual examinations lasted some four weeks. First came the written tests in literature, history, and languages, then the oral tests. I can still recall those horrible hours of waiting on the days following

each examination, in line with the others, to find out whether my mark was passing or a failure. My answers were all in the affirmative. I passed one test after another, some of the marks actually being near the top of the group.

My spirits were dampened by the profound sorrow of seeing one of my friends after another gradually dropping out, so that, finally, out of the group of forty who were admitted to the examinations, only five were left. I was particularly sad that Peisi, my bosom friend, was not even given a chance to be examined for six classes, for which he had applied.

I now became fully and officially mature. I was free from military duty, since the previous fall, on the date of my call (after reaching twenty-one), my father succeeded in proving to the military authorities that as an only son I was entitled to be freed from the military draft. I was thus at liberty to go where I pleased. But where? It was out of the question to apply to a Russian university. Since the number of admissions was limited, I could hardly compete with the day students of the Gymnasiums who had gold and silver medals, and among the Jews, it was only holders of such medals who had a chance to be accepted. Then, my mother was dead, and I was waiting from day to day to break away completely from my past.

Had I been admitted to the Odessa University, I probably would have entered it, but I did not even try. I decided to leave Russia for good. When just twenty-five years later, in the summer of 1935, I was invited, during one of my visits to the Soviet Union, to deliver a lecture at the University of Odessa, I had an opportunity to philosophize on the course of human history. Here I was in the largest auditorium of the university, packed with listeners, among whom was the now-famous Lisenko. I received a long ovation. Although I spoke only about a lowly subject, namely soil humus, I was received as the "home-town boy" who had made good. A box in the opera was ready for me. The lecture was preceded by a reception and followed by a banquet. But then, a revolution had taken place since I had left the country of my birth.

My eyes turned next to Switzerland. Many of the Russian intellectuals had gone there to study, because of the limited facilities in their own country. I had made up my mind to devote myself to the study of chemical processes of the living system. I could have selected

no better place for that type of training than the Polytechnic Institute in Zurich. I sent in my application; it was immediately accepted, and I was told to come. On considering and reconsidering this matter, I decided that this would probably mean that I would have to return to Russia in four or five years. I could hardly think about such a possibility without a shudder. With my mother gone, with the country dominated by a brutal police force, with the miserable treatment of my people, with the constant fear of life under the system of the Czars, and no apparent hope for the future—all this presented a gloomy picture. I would be another intellectual without a job, without a home.

Meanwhile, I began to receive letters from my cousins in America. They had heard of the death of my mother, who had been a second mother to them. They urged me to come to America, begin life anew, and forget the Old World. "There are as good universities here as in Europe. You will be happy here," they wrote.

I listened to their call and made my decision to go to America. Peisi decided to come along with me. His family, especially his younger sister Bobili, for whom I had developed a deep affection, would follow us later after we had become established.

We returned to Priluka in June. I spent the summer and early fall in straightening out my personal affairs. I sold or distributed among my friends my very fine collection of books, which I had gathered with so much love. I applied for and obtained a foreign passport, which would enable me to leave the country freely. By tutoring and by selling some of my mother's personal belongings, I collected enough funds to pay for my journey to the New World, plus some forty dollars extra.

About the middle of October, 1910, a group of us, three men and two women, left Priluka by train for the German border. We were accompanied to the nearest station by many friends and relatives. We were happy to leave. As our train crossed the border, we began to sing quietly, then louder and louder, until we were joined by many others in the car. Our song was the famous revolutionary, "We have shaken the shackles off our feet. We are entering upon a new world, a free world, where Man is free." The German police must have considered us as a rather peculiar crowd, although they no doubt had seen many groups like ours passing from the land of slavery into the world of freedom, where man is man.

The Promised Land

WE CROSSED northern Germany to Bremen, where we had to spend nearly a week before the departure of our ship. We took advantage of those days to familiarize ourselves with the life in an old Western European city, which was in many respects entirely different from the cities that I had seen previously. With little money to spend, we walked along the clean streets, looked into and admired the beer places, the bookstores, theaters, and historical monuments.

At last, we started on our sea journey, which lasted twelve days. The three male members of our group were perched in a bed in the third tier of a huge cabin, comprising the steerage of the ship. We shared the common joys and sorrows with several hundred other passengers. Fortunately, the weather was good. Although the food was miserable (mostly herring and boiled potatoes, or a chunk of boiled beef and some black bread), and the steerage dirty, we were happy. We were young and desirous of adventure. We spent most of the time on deck wondering at the sea and the waves that were striking at our little boat. We would gather in groups to sing revolutionary songs. Most of the passengers were young people going to the promised land and anxious to forget as soon as possible the land of oppression, the lands of the czars with its Cossacks, its jails, its limited opportunities, and its complete lack of freedom.

We finally landed on a bright fall afternoon, November 2, 1910, in Philadelphia. I can never forget my first impressions of the New World. We were met at the boat by two of my cousins, one a small businessman residing in Philadelphia, and the other a farmer, living

near Metuchen, New Jersey. Our meager luggage, mine and Peisi's, was packed in a horse-drawn carriage. We perched on top of it and proceeded through the thickly inhabited streets of the old city.

All excited at seeing my cousins who had left Priluka some fifteen years previously, we gave full reign to our amazement at everything that we saw in this new world. My cousins themselves were excited at welcoming the new arrivals from the old country, the "greenhorns," as fresh immigrants were then called. They wanted to know all about their friends and relatives. We, however, could not stop wondering at the new sights. We were particularly impressed by the large numbers of colored people, whom we had never seen before. "Now look, look, there goes a *negritionok* [small colored child]!" exclaimed Peisi. These exclamations were a source of amusement to my cousins, who repeated the story for many years. They gave little consideration to the fact that we had received a proper education in Russia, which they had not, and that we had come here to study and to escape from tyranny, rather than because we were unable to earn a living in the old country, which was the reason for their departure. We were still "green cousins" from across the seas, who had to be taken in hand and properly introduced to the New World.

My Philadelphia cousin's wife, a native American, could hardly converse with us. She prepared a hearty dinner for us, to which we did full justice. Various inhabitants of the city who hailed from our town soon began to arrive at the apartment, to greet us and to receive news from home. When we were finally allowed to take a much-needed bath and go to bed, we were more than happy.

It was my farmer-cousin's wife, Molki, who was my real cousin, the beloved niece of my mother, who often sang me to sleep when I was a child. I was most anxious to see her and her brother, who recently had been graduated in dentistry and who lived near the farm. Early next morning, the farmer, Mendel Kornblatt, and I left for Metuchen.

It was difficult for me to part from Peisi, who remained in Philadelphia, where he planned to look for some work. He spent some weeks in a pocketbook-maker's shop, then left for New York so that we could be closer together. There he obtained work in a laundry and, after saving some money, began to study to prepare himself for admission to a pharmacy school. This would give him a profession, which

would make it possible for him to earn a living while he was preparing for entrance into a university. Such was the struggle of thousands of immigrant boys who came to this country at that time.

At the Metuchen station we were met by my cousin's son, a lad of about thirteen, driving a fine horse and buggy. The town was largely residential and had that atmosphere of gentility so characteristic of many of the towns in the state before the invasion of the automobile and of industrial plants. The farm itself was on the outskirts of Metuchen, within easy walking distance of the shopping center and the railway station. My cousin Molki was delighted to see the son of her favorite aunt, who had brought her up. Molki had two children, who spoke only English, and their constant companionship facilitated my rapid command of the language and helped me avoid the sharp accents so characteristic of people who had arrived in this country at a somewhat more mature age.

Mendel took considerable interest in initiating me into the problems of farm work and farm life. He had a small poultry plant and a three- to four-acre garden where he grew truck vegetables for the local markets. Since the work was rather light during the winter months, I had ample time to read English and American literature and to make plans for entering college.

At the suggestion of my cousin, I paid a visit to Rutgers College at New Brunswick, only a few miles away. Here I met Dr. Jacob G. Lipman, also a former immigrant from Russia, who was then head of the Department of Bacteriology and who was soon to become dean of the College of Agriculture. I had a general interest in the problem of chemical reactions of living bodies, but I had only a vague idea of how to go about such a study. At times I thought that a medical school might be an appropriate place for receiving training in this field. I actually had submitted my application to the College of Physicians and Surgeons at Columbia, where I was accepted on the basis of my Gymnasium diploma. I discussed my plans with Dr. Lipman and asked his advice. Under his influence I left with the feeling that an agricultural curriculum might give me a more appropriate training in the desired field than would a medical school.

The next few months were spent on the farm, under the tutorship of Mendel. I familiarized myself with the problems of animal nutri-

tion, the handling and composting of stable manures, the preparation of hotbeds, the problems of germinating seeds, transplanting seedlings. As I watched things grow before my eyes, as I learned the principles of growth and nutrition of plants and animals, I became more and more convinced that my decision was justified.

With this in view, I applied the following May for a scholarship at Rutgers. I was allowed to take the state competitive examinations. I passed these successfully, with the exception of English literature. In spite of this, I was awarded a scholarship on condition that I pass the English requirements in September, which I did. The die was thus cast. I decided to accept the scholarship and enter Rutgers rather than Columbia.

There were other reasons for this decision. A medical training would have required large funds, which I did not possess. I could hardly have supported myself while studying medicine. My cousins themselves were struggling to make a living, and I could hardly have expected financial support from them. True, Molki's brother, Louis Kuntz, who had just established himself in dental practice, offered to help me. This, I felt, would have imposed too great a burden on him, and I was becoming more and more saturated with the spirit of the New World—a desire to depend on myself. With a free scholarship at Rutgers and proximity to the farm, I could manage my way through college, without imposing any great obligation upon my relatives. I could also feel free to leave them whenever it should prove advisable.

When college classes began that September, I went to them with mixed emotions. Here I was in a new country, speaking a new language, among boys whose lives and experiences were worlds apart from mine. For the first time in my life, except for the brief experience in the evening classes in Odessa, I was attending regular lectures, under teachers who were specialists in their respective fields.

I came in contact now with a new and unexpected form of life, quite distinct from anything I had ever seen before, and unique to one who had come with a European, especially an East European, and a small-town background. In all my previous contacts with students of equal educational and intellectual levels, I was usually among the youngest in the group. Now, I found myself at the age of twenty-three among boys who were only seventeen to twenty years old, and many

of whom were, at least in my eyes, adolescents. This was true especially when one considered their general level of knowledge, their historical perspectives, and their outlook on life. I was probably the oldest in the class, not only in actual age, but also in intellectual attainments and in variety of human experience.

On the other hand, these boys possessed far greater technical skill than I could ever hope to master. Many of them had a better understanding of the nature of government and of a form of life that comes only from having been born into a free society. My knowledge and experience came chiefly from books or from observations of a primitive, largely agricultural society, whereas theirs came from practical living. My limited understanding of the English language and my lack of appreciation of the significance of the various sports that are so characteristic of an American college placed me at a certain disadvantage, as compared to the general student body. In spite of my extensive knowledge of ancient and modern history, of several foreign languages, and of general European literature, I could have been considered by most of my classmates as a misfit who scarcely belonged to a college environment, except for my willingness to learn and my efforts to adapt myself to the new environment. After all, I was a biological system, and if there is anything that is characteristic of life and of living systems, it is the capacity of adaptation to various changing conditions.

Occasionally my classmates would joke at my ineptitude, especially at my clumsy English, at my concentration upon some subjects and neglect of others. Who could blame them for roaring when I pronounced the letters OL in a mathematical formula as "Oh Hell!"

I paid little attention to such amusements and went on with my work. Some of the subjects were quite new to me and I concentrated on them. Of particular interest were the laboratory exercises, notably in chemistry and biology, which I found not only supplemented my book knowledge, but which made possible the performance of various experiments with my hands, something that I had never done before. Scientific courses that were accompanied by laboratory experiments held the greatest fascination for me.

I shall never forget Dr. H. B. North, the professor of general chem-

istry, which was one of my most beloved courses during my freshman year. He was a man of considerable theoretical knowledge and of wide practical experience; this was based largely on a course of training in Europe under the famous French chemist Moissan. His lectures and his experiments in the laboratory were given with great enthusiasm, a characteristic unfortunately lacking in most of the other professors. He imparted to his lectures a broad historical background and a certain philosophical attitude, which attracted me at once both to him and to the subject that he taught.

Dr. North took special interest in me personally and encouraged me in various ways to come to him with my questions and my problems. He planned for me special experiments to help me satisfy my insatiable curiosity about the subject. He invited me to his home and demonstrated to me his book treasures. He greatly admired the Russian chemist Mendeleev, and, although he knew no Russian, he exhibited to me, as his prized possessions, some of the latter's work in the original language.

Among the other professors in my freshman year who attracted my particular interest was Dr. Julius Nelson. His course in general biology was not required of students taking the course in agriculture, in which I was registered. But interested as I was in living processes and their mechanisms, I asked permission to attend his class voluntarily, without receiving credit for the work. Having come once, I came again and again. I was attracted by Dr. Nelson's broad knowledge of biological problems, especially their ecological significance, his ideas of function *vs.* structure, of nutrition *vs.* growth, and a host of other reactions, which opened before me vistas never before recognized. Dr. Nelson was pleased to have in his class a student who took his course not as a requirement but for pleasure, and who tried to learn all he could about the subject.

There were certain other courses in that freshman year that attracted me. This was true particularly of mechanical drawing and analytical geometry. The first taught me the use of the line and the square, which in my inexperienced hands proved highly exciting, whereas the second offered food for my analytical mind. Unfortunately, I had little to gain from the courses that I had to take in Ger-

man, of which I already knew more than was needed, or in English and American literature, which were taught rather badly by uninspiring teachers.

I could not become enthusiastic about college athletics. To one who had learned since childhood to exercise the mind rather than the body, the various ball games appeared childish and time-wasting. I probably would have passed my four years in college without ever seeing a football game had not one of my classmates insisted that I go. He went so far as to buy a ticket for me and almost forced me to go along with him. I could not quite learn the intricacies of the game, and what interested me most was the excitement of the crowd. I watched the reaction to the game rather than the game itself. What prevented me largely from attending other games was a lack of both time and funds. After all, I had to work for my living and there was so much to learn.

In general, my first year in college brought many revelations. I learned a great deal, both in theoretical knowledge and in practical experience. My daily intercourse with my fellow students taught me a fundamental appreciation of the English language, of American life and manners, and of American history and civilization. Those professors whom I admired helped me in various ways to get all I could from their courses and also directed me toward a future goal. My continued interest in the life of animals and plants, my frequent contacts with Dr. Lipman and with several other members of the college led me to a study of the fundamental aspects of living systems, thus supplementing the practical knowledge that I was gaining meanwhile under the guidance of my best teacher of the year, my cousin Mendel, the farmer.

Mendel had left Priluka some twenty years before I left, and had later sent for Molki to become his wife. He was an expert bookbinder and, after some years as an apprentice in Chicago, had gone to Philadelphia to take a job in an important establishment devoted to the binding of rare volumes. The big city did not appeal to him. He had a longing for life in the open, for contact with nature.

Eventually he succeeded in persuading a cousin of Molki's (on her father's side), Henri Kuntz, a prominent New York lawyer, who had recently bought a large estate near Iselin, New Jersey, to make

him general manager of the farm. Molki took charge of the house, where Henri and his brother Charles, both bachelors, lived, while Mendel attended to the cultivation of a large part of the hundred-and-fifty-acre farm. He gradually learned the art of growing vegetables, of raising chickens and other poultry, and of attending to cows and horses. He did it all with the minute diligence which he was accustomed to impart to the binding of books. He read a good deal, so that his knowledge of farm problems was both practical and scientific.

Henri was chiefly concerned with his legal practice in New York, which apparently brought in considerable sums of money. This was largely spent in supporting the education of several immigrant boys and in enabling Charles to carry out an educational experiment known as the "Ideal Youth." A group of boys and girls, children of relatives and friends, were used for this. In the end it failed, as have many other idealistic ventures based upon impractical outlooks upon life and environment.

After caring for the big house and farm for several years, my cousins grew tired of the excessive work and longed to have a home of their own, where they could raise their two small children. This they accomplished by the purchase of a five-acre farm near Metuchen, only six miles away. When I arrived on the scene, they had been settled for only a year or two in their new home.

Mendel was my first real teacher of farming. He taught me not only the art of growing vegetables and raising chickens, but also the appreciation of the fundamental principles of growth of plants and animals, as influenced by conditions of environment, by nutrition and heredity. In preparing the manure heap for the hotbeds and cold frames, he taught me the essential principles of aeration in the decomposition of plant and animal residues, the effect of the composition of animal excreta and bedding, the role of a proper moisture content, and the influence of other factors upon the production of a suitable compost. He taught me the importance of proper selection of seeds, whether they be those of the tomato, bean, corn, or other crops, that go to make up a vegetable garden. He directed my attention to the marvels of nature involved in such commonly accepted facts as seed germination, plant growth, and crop maturity. When he lovingly collected the eggs from the several pens, each made up of six hens and

one rooster, and when he placed these eggs under a sitting hen or in an incubator, he pointed out to me the essential principles involved in growth and nutrition of the young chick, whether under the natural care of the mother hen or under the artificial care of the brooder house. He emphasized, better than any book or any professor could, what it takes to obtain a healthy chicken, whether she is raised for yielding eggs or for use as meat at the table. In fact, at the end of my first year, under Mendel's tutelage, I submitted to *The Rural New Yorker* an essay entitled "How I Raised a Flock of Chickens," and received for it my first ten-dollar award.

Mendel not only initiated me into the mysteries of plant and animal life, but he gave me the basic knowledge for my college training in agriculture, the desire to learn the fundamental principles and the chemical and biological mechanisms that make agriculture possible. Before coming to America, I had had only a vague idea about plant and animal life. Although I wanted to make my lifework the study of chemical reactions of living systems, living reactions were known to me in an abstract rather than a concrete form. Under Mendel's guidance, however, I came to familiarize myself firsthand with living processes, comprising both plant and animal forms, in their numerous manifestations and under various conditions. The practical knowledge of plant and animal life that I thus gained made me even more anxious to study the underlying reactions, so that I could comprehend not only what life is as a whole, but what its manifold manifestations are and how they can be modified and controlled.

To make such knowledge more comprehensive, I needed to know first of all the numerous chemical and microbiological processes that go on in the soil and that result in the liberation of a continuous stream of nutrients which make possible the continuity of life and which serve to complete the chain of living reactions in nature. It was to the soil, therefore, that I decided to go for an answer to the many problems that had begun to puzzle me about the cycle of life in nature. This gradually led me to the study of the microscopic population of the soil, its role in soil processes, and the biochemical activities of microorganisms, problems that were to become the crowning glory of my research career.

Mendel thus laid the foundation for my early education in the

science of living reactions. Some of my professors, notably those who taught me chemistry, botany, zoology, and bacteriology, helped me to gain the basic facts. I should also name particularly Dr. Byron Halsted at Rutgers, who helped complete my training in the appreciation of the cycle of life in nature and the role of microorganisms in the complex chemical and biological processes. Some of my friends during my graduate training, notably R. E. Curtis, D. C. Schultz, and R. E. Cook, and later my own students stimulated in me further the spirit of research in this direction.

I spent on the farm in Metuchen the first two and a half years of my life in America. This arrangement had the particular advantage that after classes were over I could come back every day to a home with a friendly environment. I helped Mendel with his work on the farm, in the building of his poultry houses, in the feeding of his flock, in attending to the garden, and in a variety of other farm chores. I was thus able to earn my keep. This sort of life had the disadvantage, however, that it gave me little opportunity to come in closer contact with my professors and with my fellow students after my classwork was over. I was thus unable to participate in extracurricular activities. I also missed the time that I would have liked to devote to reading in the library. I was also anxious to spend more time, in addition to the required hours, on some of my laboratory work. All this was quite impossible while I had to lead the life of a commuter. Then, too, I began to feel a need for some extra funds, which I could not earn while living on the farm.

I decided, therefore, at the end of my sophomore year, to leave the friendly farm and move to New Brunswick. I inquired about and was at once offered a job, first for the summer months and later during my spare time the rest of the year under the botanist, Dr. Halsted. This involved taking care of some experimental work in the greenhouses and fields at the College Farm. On cold or rainy days and in the evening, I helped with laboratory experiments. This turned out to be one of the most important decisions in my life. It was a logical step for me, to proceed from my knowledge of practical farm operations to the study of plant nutrition and plant genetics. I had the added advantage of working under the leadership of a master botanist who not only guided me in my first experimental work but who also en-

couraged me in many ways to learn every aspect of the problem involved in the study of heredity, structure, and function of living systems.

There was another reason for my leaving the farm at that time. Bobili, Peisi's young sister, who was later to become my wife, had recently arrived (in February, 1913) from Russia to join her brother in New York. It would have been rather difficult for me, while still living on the farm, to take the necessary time and spend the necessary funds for the frequent journeys to New York that I was planning. Only by having an independent source of income could I hope to use the time and gain the money needed for that purpose.

In June, 1913, I packed my few belongings and moved to a simple room which was placed at my disposal in an old farmhouse on the College Farm grounds. A new life thus began for me. The environment was ideal. I earned enough to live modestly and to continue my studies uninterrupted. Most important, I could use my spare time as I liked. My compensation for the work was only twenty cents an hour on weekdays and thirty cents on Sundays. The fact, however, that I paid only three dollars a month for the room and could purchase on the farm cracked eggs at eleven cents a dozen helped in balancing my meager budget. I also managed to obtain, in addition to the regular employment, several supplementary and more lucrative jobs. One had to do with the replacement on Sundays of the two caretakers of the Poultry Department, thus giving me an additional two dollars a day for a bare four or five hours of work. Occasionally, I also replaced the night watchman at the farm, thus bringing in an additional sum of three dollars a month. The best remuneration of all came to me, however, from teaching English to various foreigners living in New Brunswick or from preparing an occasional student for his college entrance examinations. My previous teaching experience in Russia was of considerable help to me, since I knew exactly what the needs of these people were and how they could accomplish most in the least time. All these sources of income thus made it possible for me to pay all my college expenses, buy occasional pieces of clothing, and also render some help to Peisi, who was at that time working very hard on his pharmacy course in New York. I managed, as well, to visit New York at various opportunities.

My sophomore year in college had proved to be rather disappointing, for my chemistry professor was an unimaginative bore, and my biology teachers had been uninspiring. The excellent course of general chemistry that I had had during my freshman year, followed by a very mediocre course in qualitative analysis during the second year, had made me hungry for more chemistry, both quantitative and organic. The uninteresting courses in English and American literature in my freshman year had been followed by an even worse course on Shakespeare in my sophomore year, thus completely discouraging my appetite for literature. In fact, it took me more than ten years to overcome the aversion that the course on the Bard of Avon had aroused in me.

A very poor course was offered in general soils. It was taught by an instructor who was more interested in sports than in his classes. For teaching, he was none too well prepared.

The course in physics had proved to be almost as great a disappointment as that in qualitative chemistry. The chemistry professor actually had said to me: "It is nonsense to believe that there is much to this dissociation theory of Arrhenius; this concept is as ridiculous as that of the transmutation of elements." I must have completely disgraced myself in his eyes, for I believed in such theories and did not hesitate to express my opinion. I failed to convince him, however; what I succeeded in was receiving a D grade, the lowest in my whole college curriculum. The physics professor, a charming old gentleman, who should have been retired many years before, knew much better, but he should not have been allowed to continue teaching physics to young minds, some of whom might have made some use of the subject. I had picked up enough facts, however, from the laboratory experiments in that course to feel that my time had not been entirely wasted.

The course in French was given by an enthusiastic teacher, but to one who was thirsting for scientific facts and ideas this proved a poor substitute, especially since I felt that I had enough knowledge of foreign languages.

The only saving features during my sophomore year were the courses in zoology and in botany. To be sure, they were not taught by overenthusiastic teachers, but the experimental approach they unfolded had served to arouse great interest and gave me some training in fields for which I was preparing.

It is no wonder, then, that I was looking forward to my third year in college, to correct the failures of my sophomore courses and to open new perspectives for a future scientific career. Moreover, I now felt economically secure and could devote all my spare time to my studies. My hopes were fully justified. The teachers in my junior year, not only of the sciences but also of the humanities, brought light and wisdom to my mind.

That year I took my first course in general bacteriology under Dr. Lipman, who later also taught me soil bacteriology. He was a leader in the field. I felt that I finally was under the tutelage of a master. Dr. Halsted, whose assistant I had now become, offered no formal courses, but in our daily contact, whether in the greenhouse, in the field, or at the laboratory bench, he taught me the fundamental laws of genetics and of plant nutrition and the principles of scientific thought. The other courses that year, such as entomology, plant pathology, and quantitative chemistry, were taught by earnest men who tried to impart not only basic knowledge but also a certain amount of philosophy in the respective sciences.

Among the humanities taught that year was the history course given by the genial and philosophical Prof. John Logan, who was far ahead of the average college teacher of those days in his appreciation of world problems. The course in economics was taught by the austere Prof. John Scott, who never failed to emphasize the economic development of America and the future course that it was bound to take, especially in problems of capital and labor and in state and federal relations. That year, when the first World War broke out, the guidance of both these men helped me greatly in trying to adjust my mind to the course of world events and to the potential and probable role that was to be played in world affairs by my new, adopted country.

There were other professors who contributed much to my education. I certainly should not fail to mention Dr. E. L. Barbour, professor of elocution. He took infinite pains in teaching me proper enunciation and diction. When my fellow students would laugh at my exuberance in presenting a public address before the class, he would caution them: "Boys, boys, do not laugh at Waksman. You may have the advantage of the language, but he is earnest and understands what he is trying to

present!" Prof. R. J. Wright, who later taught me organic chemistry, was a wise counselor.

I had but little direct contact with the president of the college, Dr. William H. Demarest. He impressed me as a strict disciplinarian and was certainly not as sympathetic to some of my efforts as I had hoped he might be. The following may serve as a case in point. I tried in my junior year to organize at the college a chapter of the Menorah Society, which was devoted to a study of Jewish culture and Jewish problems, a subject close to my heart, but in which there was a complete lack of interest in the college, in spite of the fact that the student body contained a fair proportion (some fifteen per cent) of Jewish boys. When I presented a plan for organizing such a society to Dr. Demarest, he was not only unsympathetic, but expressed fear that this might be the beginning of a new fraternity. Only after I had convinced him that membership in this organization would be open to the entire student body and that the learned Dr. John Raven, professor of Hebrew at the Theological Seminary, would be our adviser, did he agree.

One incident of my junior year pleased me greatly. As I have said, I was looking forward eagerly to the course in quantitative chemistry. All students in the agricultural curriculum had to take this course in their junior year. Since it involved assiduous labor and since the agricultural curriculum was looked upon by many of the students as a "cinch," the preceding classes had been applying to the faculty for permission to drop this course and to substitute other courses, such as agricultural economics, which would require less effort. To my complete disgust, the faculty decided to omit this course from the curriculum beginning with my class. This upset completely my future plans for a thorough training in chemistry, especially after I had wasted so much time the preceding year in qualitative chemistry.

The decision of the faculty caught me quite unaware. Since the courses were rigorously fixed, I could not apply for permission to substitute the chemistry course for any of the other courses. The best that I could do was to ask for permission to take this course without credit, but this was no easy matter either. I had to put up a terrific fight before the faculty. First, I appealed to my classmates to come along with me in my application. Most of them considered this as too funny for words

and merely laughed at me. But to my great surprise, two of my fellow students agreed to go along with me in my efforts. We submitted a formal application to be permitted to take this course on our own time, in addition to the other prescribed courses. Such permission was finally granted, and we were allowed to take the course with the regular chemistry students, but without receiving any college credit for the time thus spent. This action of the faculty was quite satisfactory to us, since we had feared that the course would involve extra fees, which none of us could afford at that time. Thus, for a whole year, we spent ten to twelve hours a week in the laboratory, without receiving a single unit of credit. We were exceedingly happy, however, and so was our professor, the genial Dr. R. O. Smith, who had ample cause to be disgusted at the action of the previous classes in the agricultural curriculum.

I may add here that I was considerably ahead of my class and was, therefore, allowed to carry several extra courses, which made it possible for me to complete nearly all the required senior work by the end of my junior year. Although I was not permitted to graduate a year earlier, I was able to devote most of my time during the senior year to a research problem. I was the only student majoring in soil bacteriology, and Dr. Lipman could hardly plan a separate course in this subject for me. It was more convenient for him to place me with a group of graduate students who were gathered around him at that time. They were serious men who came from various universities of the country to specialize in a field of science for which the New Jersey Agricultural Experiment Station was already famous. There was a young instructor who had recently been appointed by Dr. Lipman to assist him with this course. He was not research-minded, but he knew well the subject of general bacteriology. Since I was the only student assigned to him, he spent most of his time helping me in preparing media, washing dishes, plating soil, isolating and identifying organisms, and other routine chores.

The special problem that I selected for my senior work was concerned with the enumeration of different groups of microorganisms—bacteria, fungi, and actinomycetes—occurring in the soil. Each month, throughout the year, I dug several trenches at different parts of the College Farm, to a depth of thirty or more inches, and wide enough for

one to step in. Although I had had a course in soil physics the year before, I knew little of the soil structure and less of soil formation. Still, I could not help but observe the several distinct soil layers, which I learned much later to designate as "horizons." From the different layers, I took samples of soil under aseptic conditions. These were brought to the laboratory and carefully sieved. Weighed portions of each soil sample were suspended in sterile water and plated out on special culture media. After incubating the plates for two to seven days, I examined them carefully and counted the colonies that had developed. At first, my main purpose was to count the bacterial colonies only. Here and there, however, there appeared also fungus colonies. I counted these as well. To my amazement, the agar plates also showed small colonies of organisms which were similar to those of the bacteria but under the microscope looked very much like those of the fungi. These colonies appeared to be conical; they were leathery and compact when touched with the needle, and frequently pigmented when isolated and grown on different media.

I immediately drew Dr. Lipman's attention to the great abundance of these colonies and asked for suggestions as to how to characterize and classify them. He confessed that he had never paid much attention to such colonies and considered them as some sort of bacteria, perhaps "higher bacteria." I appealed then to the plant pathologist, Dr. M. T. Cook, who taught me botany and mycology. We examined the colonies again and came to the conclusion that they represented an obscure group of little-known organisms, which usually were designated by the name *Actinomyces*. At the end of the year, I tabulated my results. To my great amazement, these organisms showed a decided regularity of distribution in the soil. Their numbers depended entirely upon the nature of the soil, its reaction, depth from which the sample was taken, and the crop grown. Thus began my interest in a group of microbes to which I was later to devote much of my time and which were to remain for the rest of my life as my major scientific interest.

While I was becoming more and more interested in soil microbiology, my work with, or rather for, Dr. Halsted was also making progress. In addition to earning most of my living expenses from this work, I also gained considerable knowledge of plant genetics, or the hereditary properties of plant life. I devoted my time now to the breeding of

different vegetable crops. I was thus able to gain an insight into the Mendelian concept of heredity and the great complexity of inherited characters. Dr. Halsted was well pleased with the results of my work. He entrusted more and more to me those particular studies in which he was personally vitally interested. He spent considerable time discussing with me not only the results of these experiments but also scientific problems and fundamental principles as a whole. He would sit with me for hours, often a whole morning, if it happened to be on a Saturday, or an afternoon when I was free from classwork. He would discuss scientific methods, compare the American with the European approach to a scientific problem; he would analyze science from the point of view of the teacher and investigator, and would compare various colleges, universities, and special research organizations which were known for their work in botanical and allied sciences. He himself had had contact with some of the most outstanding American botanists and had devoted a lifetime of research to the nature of plant life, plant diseases, and plant protection. He analyzed for me the advantages and disadvantages of different institutions. It may be said that he opened before me the gates of the American university, notably those that would be most promising to a young scientist. He would say: "Do not be attracted by the glamour of the large institution, the luxurious laboratory equipment, but by the mind of the investigator under whom you want to work. Find him and go to him no matter where he is."

Having thus spent many hours with me, Dr. Halsted would leave and add with a smile, threatening me with his finger: "Now, do not fail to report these hours as having worked for me, otherwise I will never talk to you again." He knew very well that I could ill afford the twenty cents an hour that I would have lost during the time that he had spent educating me, helping me to broaden my horizons, and instilling in me the true scientific spirit, the desire to search for the unknown, and the exact methods to be used in such a search.

I owe much to him, to his scientific mind, to his great generosity. Here is another illustration. Just before graduation I was elected to Phi Beta Kappa, the honorary scholastic fraternity. Since my classes were over, I came early the next morning to the laboratory to report for work. Giving me the fraternal grip, Dr. Halsted took from his

pocket his own Phi Beta Kappa key and showed it to me. A Quaker by religion, he never wore decorations. He then handed me a five-dollar gold piece and added: "Please take this downtown to the jeweler, tell him of your initiation, order a key for yourself, and let him send me the bill for the remaining amount; this is my graduation present to you." I was so overcome with emotion that I could only utter a few words in reply, to the effect that I would deem it a privilege to wear this key all the rest of my life, as much in memory of him personally as of the society that was thus honoring me.

When our son was born in September, 1919, Dr. Halsted had just recently died. Since Bobili was also devoted to Dr. Halsted, having worked one summer for him as an assistant, we could do no less than name our son, after him, Byron Halsted.

Before leaving my undergraduate college years, I must emphasize that I was not in a true sense what most of the students called a "bookworm," in spite of the fact that I had spent considerable time reading books on a variety of subjects and studying both in my room and in the library. Although I did not care for physical sports, I liked to participate in various college activities, such as the chess club, the college paper, and literary discussion groups. I participated actively in several cultural organizations, having helped to organize, in addition to the Menorah Society, a Chapter of the Intercollegiate Socialist Society, now known as the League for Industrial Democracy, for the study of social, economic, and political problems facing the world at large and the United States in particular.

I could not quite stomach the childish outbursts that found expression in the numerous college cheers, perhaps because I was forced by the upperclassmen to learn them during my freshman year. I did enjoy the college songs, however, the debates held between various college groups, and other forms of intellectual exercise.

Although I had to take military training all the four years that I spent in college, I was probably the worst soldier in the battalion. Not long ago, in fact, I was greeted by the former corporal of my squad: "Waksman," he said, "I well recall, when I commanded, 'Squad right,' that you kept going forward; I said then, 'What a fool you are.' I say now, 'Perhaps you were right and we were all wrong.'"

In June, 1915, I received my Bachelor of Science degree. The ques-

tion was, what to do next. Should I look for a job or should I continue my education for a higher degree? Dr. Halsted offered me an appointment in his department, but my interests were not primarily in the field of botany or genetics. The soil and its life, especially my new-found friends, the actinomycetes, were just what I had been dreaming about my whole life. Should I then continue at Rutgers or should I proceed to a large university to take more advanced training in the fundamental sciences, which I knew I badly needed, before I could go much further in my own selected field of research? There was no question about my interests. There my mind was made up. The question was merely whether I should pursue that training now, or a year from now, after I had had a chance to dig deeper into the soil and establish myself somewhat more firmly. Dr. Lipman advised the latter course, and I finally accepted an appointment as assistant in his department, with an opportunity to work for a Master of Science degree at the college.

Thus on July 1, 1915, I became a research assistant in soil bacteriology at the New Jersey Agricultural Experiment Station, an institution with which I was to be associated practically my whole future life. There were two of us in that soil bacteriology laboratory on the top floor of the administration building, overlooking the city reservoir. The other was Curtis, a graduate of Oregon State College, an earnest and capable worker, who was to become my good friend.

Dr. Lipman, with little time for supervision of our work, left us almost entirely to our own resources. True, he assigned to each of us a specific problem. I was to study the effect of protozoa upon soil bacteria, and Curtis was to characterize the bacterial population of the soil and establish certain well-defined types. Neither of these problems had much attraction for us. I soon became tired of the fruitless search for pure cultures of protozoa and gradually drifted to the study of soil fungi and actinomycetes, two groups of organisms that had left a marked impression on my mind since my senior problem. Curtis also lost interest in his task. After watching me for some weeks and helping me with my work, he gradually joined me in the study of actinomycetes. Happy that we were making progress, Dr. Lipman paid scarcely any attention to the change in nature of our respective problems, or if he did, he said little about it.

My first year's graduate work may thus be considered as a direct continuation of my senior problem. There was very little change, either, in my mode of living. I was now somewhat freer in carrying out my research work. I was receiving a monthly stipend of fifty dollars, which was more than sufficient to cover my living expenses, and I did not have to look for additional outside work. I continued to live at the farmhouse on the campus. All my time, except for an occasional period devoted to some training in organic chemistry, I could spend in the laboratory, which meant from seven o'clock in the morning to eleven at night. "The light in those windows never goes out," said the manager of the experimental farm. "When I go to bed at night, it is still on; when I get up in the morning it is already on, or has it ever gone off during the night at all?"

That year I had two good and true friends on the campus and two in New York. Curtis, my co-worker, usually arrived every morning at the laboratory laden with food which was to nourish us the whole day. After a hasty breakfast, we would start making up media, washing dishes, plating soils, isolating cultures, transferring cultures, examining cultures, testing, weighing, analyzing, and carrying out the thousand and one things that go with bacteriological manipulations. We took only a few minutes for lunch and continued on, except for those days when we went to the organic chemistry laboratory. Occasionally we went to town in the evening to have dinner, then returned to the laboratory for another two or three hours of work.

My second friend on campus was Schultz, a graduate of Iowa State College, who had taken the position of assistant to Dr. Halsted. He was of German-American origin and was educated in the spirit of true romanticism. He was a gentle boy, full of ideas about human relations, poetry, philosophy, and all that goes to make human life broader and fuller. He awakened in me my old literary interests. We would spend many an hour arguing about the war, about Ibsen and other dramatists, about Hegel, Kant, and other philosophers. He did not allow my scientific interests to enslave me. He was a living symbol of my past experience and of a potential broad future. The other graduate students, and there were some eight or ten of them, were interesting to meet for discussion of our scientific problems, but none became so close to me as either Curtis or Schultz.

This was a most interesting life, a most exciting life. There were certain important interruptions to this routine, such as my weekly or biweekly trips to New York to visit Peisi and Bobili. That fall I went to Washington to spend several weeks with Dr. Charles Thom in the laboratories of the U.S. Department of Agriculture, to learn methods of studying green penicillia and other fungi. Here I established a lasting friendship with a great scientist, which was to bear fruit in the years to come.

Time was passing very rapidly. Before long, I had an opportunity to write my first scientific paper, which was presented by Dr. Lipman before the Society of American Bacteriologists, at its annual meeting in Urbana, Illinois, at the end of December, 1915. This paper, "Bacteria, Actinomyces, and Fungi of the Soil," was never published as a whole. Only the abstract appeared in the first number of the first volume of the *Journal of Bacteriology*. Soon after, in February, 1916, my first complete papers were published. These dealt largely with my investigations on the fungi and actinomycetes of the soil. I began to receive letters of encouragement from various investigators throughout the country and even offers of jobs. The academic year was rapidly coming to an end. With it, came a Master's degree and a departure from New Brunswick.

Curtis remained for a while, continuing our investigations on the isolation and identification of actinomycetes. I took along with me transfers of all the cultures that we had already isolated and described. I was hoping to be able to continue my work with them; I wanted to make sure especially that they were kept alive. It was well that I did. Although Curtis collected a mass of new data on actinomycetes, he lost all interest in the problem some months after I left. Moreover, when we entered the World War, he joined the Army as an instructor in the management of trucks and tractors. He left the laboratory, never to return to it. When I came back to New Brunswick in 1918, I found all the cultures dead, and the laboratory in a state of confusion. The drawers were full of notes, which I could barely decipher. I saw Curtis again a few times after my return, but his interests had shifted from the pursuit of scientific investigations to practical farming.

Schultz also disappeared from my life. When the war broke out, he was drafted into the Army and spent most of his time in several hospi-

tals. When released, he completed his training in botany at the University of Chicago and took a teaching position in the West. We exchanged a letter or two, but our paths never crossed again.

Thus came to an end my preliminary preparation for a research career at Rutgers. I had made a few good friends, I had obtained a great deal of basic training, I had sent my roots into the soil in search of its microbiological population. I was now on my way. I knew now exactly what I wanted and how to get it. The rest was merely to follow a plan. California was to prove whether I was on the right track.

California Interlude

BEFORE I set out definitely on a research career, it was quite necessary for me to obtain supplementary training in the field of chemistry, especially in biochemistry. This could only be obtained by going to a large university to study for my advanced degree. With this in view I selected three institutions, where there were outstanding investigators who could offer me the type of training that I was interested in and also direct me in my research work. Since proteins and their transformation in the living system were most important for a proper understanding of the metabolism of microbes, and a knowledge of proteins offered some of the greatest opportunities in the solution of microbiological problems, I was naturally attracted to those investigators who devoted most of their attention to this field of chemistry. I thought first of Drs. Thomas B. Osborne and Lafayette B. Mendel at Yale University, both of whom had contributed greatly to our knowledge of plant proteins and protein metabolism. I thought next of Drs. A. P. Matthews and F. C. Koch at the University of Chicago; finally of Drs. T. B. Robertson and G. N. Lewis at the University of California. The last group appeared to be opening new horizons in chemistry as a whole and in that of proteins in particular.

I was short of funds, since I had been able to save but little from my meager salary during my year's graduate work at Rutgers. In fact, my total savings at the end of the year were about fifty dollars, an amount just about equal to what I had brought with me from Russia. I could not dream of going very far without assured financial support. With this in view, I made applications for fellowships to the above

three institutions. Fortunately, the February issue of the journal *Soil Science*, containing my first paper on the "Actinomyces of the Soil," appeared just in time to support my application. Apparently, this publication, as well as the fine recommendations sent by my professors at Rutgers, made a good impression, since I was offered fellowships wherever I applied. It was for me, therefore, to make the choice. For the reasons given above and after a careful comparison of what the institutions as a whole had to offer me, not the least important being the amount of money attached to the fellowship, I decided to go to California.

In responding to my acceptance of the fellowship, Professor Robertson wrote that although he knew very little about microbiology, he felt that he could suggest various leads in the fields of biochemistry and physical chemistry, notably in the structure and transformation of the protein molecule by enzymes, both of which might help me in obtaining a better understanding of the role of proteins in microbial nutrition and in their degradation by microorganisms. Thus encouraged, I made all preparations for leaving New Jersey about August first to go directly to Berkeley.

Since the academic year at California began about the middle of August, I had little time left for rounding up my scientific work. There were the various cultures of actinomycetes and fungi that had to be carefully selected and transferred, some to be taken along and some to be left in New Brunswick. There were the final chemical analyses of some of the experiments and the various recorded observations to be completed, the computations to be made, the reports to be written, including two papers on my fungus work.

My Master's examination took place in June. It consumed little of my time, however, since I knew both the major (soil microbiology) and minor (organic chemistry) subjects well, whereas the thesis itself formed a part of the larger scientific program. The last two months in New Brunswick were, therefore, spent in intensive laboratory work and in writing up my results. I probably worked at that time at the rate of fifteen hours a day, including week ends and holidays. Finally, all the experimental work was completed, the papers were written and submitted for publication, and the laboratory was cleaned up for the occupant who was to take my place.

Another major problem that had to be decided at that time was my marriage to Bobili. Although she preferred to postpone it for another year, my going West brought our decision to a head. Unfortunately, she had saved no money from her work in New York and it was necessary to provide transportation for both of us. Peisi and another brother, Hershel, who lived in Colombia, South America, loaned us two hundred dollars. This, together with my limited savings, my final month's salary and that for a month's vacation to which I was entitled, helped us solve our financial problems. It took very little time to prepare our departure.

All our belongings were packed in two bags, so that I could easily handle them at the railroad stations, where we made our stops, without having to pay for the services of a porter. All my books and papers were packed in a trunk and stored on Mendel's farm. Although my work at the Experiment Station was officially completed on July 31, I continued my experiments virtually until the last minute of my departure.

On the morning of August 4, I left for New York and called for Bobili at her room. Dressed in our best, we proceeded to the courthouse, where we obtained the necessary marriage papers, which we dutifully signed, paid our two-dollar fee, and were officially pronounced husband and wife. This ceremony was no more complicated than the one that had taken place five months before in the courthouse in New Brunswick which pronounced me a citizen of the United States. I was proud of both, especially since neither was accompanied by any pomp or parade.

After the official ceremony was over and the marriage certificate tucked away, Bobili and I had lunch in a small restaurant in downtown New York and arranged to meet the next evening about six at the Twenty-third Street Lackawanna Ferry, which was to take us on the first leg of our long journey westward. I returned immediately to New Brunswick, to pack my own things. The rest of that day and night and the following morning I spent in sealing and packing the actinomyces cultures, with instructions to have them forwarded by mail to Berkeley.

On the eve of my departure, all of my friends, including Curtis and Schultz, gathered in the laboratory to bid me farewell. There were

no formal speeches, since all of these had been delivered three months before at a dinner given to some six members of the faculty by the graduate students, of whom there were about fifteen at the College of Agriculture. The last evening was, therefore, spent without oratory and without drinks. It was more like a round-table discussion of each one's plans for the future, of the fields of science or teaching that each was interested in. We were all disturbed by the problems which we knew each one of us was to face in the oncoming war. The United States was not at war as yet, but we all felt that its coming was quite certain. We parted most cordially. They were a fine group of men gathered at that time at the New Jersey Agricultural Experiment Station.

On the afternoon of August 5, Curtis and Schultz saw me off at the station in New Brunswick. We shed no tears, but we were close to them. Upon my arrival at the ferry in New York, I was met by Bobili, who was accompanied by Peisi and a friend. These two went with us to Jersey City and saw to it that we were comfortably placed in a Pullman car, with Buffalo as our first destination. The trip was so arranged that we could travel at night and stop in the daytime. Thus we could save on hotel expenses and see as much of the country as our time and limited finances would permit.

When the train pulled out of Jersey City and we waved farewell to Peisi, there was a lump in my throat, although we were fairly certain that before long we would be together again in California.

Our trip, which combined a sight-seeing tour, a honeymoon journey, and a quest for knowledge, naturally proved to be highly exciting.

Our first stop was made at Niagara Falls, where we spent an unforgettable few hours admiring the scenic beauties. The second night we were again on a train, rolling over the Michigan plains. We were so tired that we rested despite the stifling heat and the stuffy and uncomfortable sleeper. The second day, in Chicago, turned out to be a scorcher, with temperatures approaching one hundred degrees. Nevertheless, we went on our pilgrimage to the university, where I found Dr. Koch, the biochemist, and expressed my sincere appreciation for the fellowship that had been offered to me and my regrets for having refused it. I was received very courteously, shown through the laboratory, and told about some of the work going on there. The rest of the

day we tried to keep cool in Jackson Park. The third night the train again rolled through the heat, this time over the plains of Iowa and Nebraska.

The Western plains and mountains proved to be the wonderland that we expected. After a stop at Colorado Springs, where we depended largely on our legs for sight-seeing, and at Salt Lake City, where we admired the evidences of Mormon civilization, we crossed the Sierras, arriving late one day in the middle of August at Berkeley, California. The station, which was outside town, looked bare and unfriendly. Here we were, alone at the edge of the continent, if not at the end of the world, a bewildered young couple with only two small bags to our name. Through someone we knew in New Brunswick, we had arranged to have a young lady, Theresa Bernstein, from New York, who was visiting Berkeley, obtain for us a small furnished apartment and meet us at the station. We were brought to a most convenient furnished apartment in a small bungalow, costing us fifteen dollars a month plus fifty cents for gas and electricity. Our first dinner was arranged for us at the home of a friendly family. Miss Bernstein even saw to it that we had some food stored away in the ice box in our small apartment for the next morning. We never met her again and never could express our full appreciation for her help in lightening the difficulties of our new environment.

California extended a welcome hand to us wherever we turned. The people there were far more friendly and anxious to help us than were people in the East. The owner of the bungalow helped us get settled and arrange things to our satisfaction.

We spent the first morning marketing. To our great delight, food, especially fruit, was plentiful and cheap, but even so, we realized that, since the income from my fellowship was only about fifty-five dollars a month, either Bobili or I would have to get some extra work to supplement our meager resources.

That afternoon I went to the laboratory, where I was to spend my next year and a half, and was welcomed by Dr. Robertson and his associates. I was at once assigned a space in the laboratory and was told to go to work. It did not take us very long to settle down to a strenuous, but friendly, routine.

Our coming to Berkeley opened a new chapter in our lives. First

of all, there was the university itself. Everything here was so different from all my experiences in Odessa, on the one hand, and at Rutgers, on the other, that I can truthfully say we had entered a new world. This was a truly great university. The variety of courses in the many departments, of which heretofore I could only dream, was offered by a staff of professors who were outstanding investigators and philosophers. The cosmopolitan student body and the coeducational system were new to us and tended to present a totally different aspect of educational life in general and of American life in particular.

Most important of all was the fact that the laboratory to which I had come was headed by a man who could supply me not only with much-needed information and guidance, but also with a new scientific approach and a new scientific philosophy. I should like to quote from one of Robertson's public addresses, which he delivered at that time, to emphasize what I have in mind. He said:

I earnestly hope that every student of science will become an ardent devotee of research and exponent of the spirit of research, for the spirit of research is that spirit which inquires for the purpose of making things better than they are, and which urges humanity toward higher purposes and more worthy achievements in every aspect of our lives. What, after all, is really worth doing in this life? If our object be merely to keep things going as they are, then, truly, all the activities of mankind become virtually nothing more than housekeeping on a world-wide scale. We would grow food today that we might eat tomorrow, make clothes solely in order to wear them out, pass our lives in absolute subservience to our animal needs, earn merely what we spend and for spending's sake.

Tall and rather clumsy, Robertson had a striking personality. He was still young, only thirty-two or thirty-three years old, and was apparently greatly influenced by his wife, a charming, intellectual and artistic woman. He was interested in protein chemistry, in the mode of action of enzymes, in the nature of hormones and of cancer—all problems from which I could have benefited greatly. An inspiring teacher, he interspersed his lectures in biochemistry with interesting personal experiences.

Robertson was himself a physical chemist turned biochemist, under the inspiring influence of his former teacher and guiding spirit, Dr.

Jacques Loeb. The latter considered Robertson his most gifted student, and, when he left the University of California, he recommended that Robertson be appointed in his place to the chair of biochemistry. Robertson was still young enough to have continued to benefit from the wise counsel of his teacher.

Robertson undertook a number of problems which were later to revolutionize our biological thinking. Those problems required a large group of collaborators: a single investigator, no matter how brilliant and how hard-working, could make no more than a dent in most of them. Unfortunately, Robertson did not appreciate this. He was completely unable to make use of his own students, whom he considered mostly as nuisances. He attempted to solve his many problems with the assistance of two or three technicians. When I came to work with him, I was prepared to take up any problem in the field of biochemistry that he would assign to me. I would have liked nothing better than to work on one of his problems. He expected me, however, to suggest my own problem and to limit myself to receiving only very general advice from him. The result was that here again, as at Rutgers, I was largely thrown on my own resources and was forced to select my own research approach. Had Robertson been able or interested enough to attach me to one of his own problems, which would have led into the field of pure biochemistry, probably that of enzymes or of hormones, this might have been one of those turning points in my career that I mentioned previously.

I decided, therefore, to continue with the study of the fungi and actinomycetes, the organisms that I now knew so well. I tried, as best I could, to concentrate on their biochemical properties, particularly their production of proteolytic enzymes. I was thus successful in combining my knowledge of microorganisms with the application of the methods developed by Robertson for the analysis of their biochemical activities. Unfortunately, I seldom had a chance to discuss my problems with him in detail. He was too self-centered, too much devoted to his own work to find time for, or to show interest in, the work of his students. He complained that he was becoming too popular, and finally decided to see his students only once a week during a definite two-hour period set aside for this purpose.

Robertson could himself be designated as a "mechanistic philoso-

pher," an expression which he pinned upon Loeb. A living system represented to him nothing more than a series of tropisms, or reactions to stimuli, whether external or internal (hormones). Although we never discussed the significance of spiritual values and social concepts in formulating or influencing historical movements, I doubt whether he gave these much thought. Having been born in Russia under the Czars, I could well understand why a revolution, once it came about, had to take on so many extreme forms. It was by nature and historical circumstances bound to be not only political, but also economic and social. We had come a long way from the French Revolution of a century and a half ago. I could hardly sympathize with most of those who escaped from their homeland and ran away from conditions that they were partly responsible for creating. These emigrants did not come to America, as did most of us earlier emigrants, to help build a new world, suffer for it if necessary, and benefit from what their labors would help to create. They came merely because it was easy to come here; they were mostly desirous of benefiting from a state of society to the building of which they had contributed little. But Robertson and his friends were in no position to analyze the great historical events that transpired before their eyes. To them, the revolution merely meant that the new Russia had dropped out of the ranks of the Allies.

Another teacher who influenced me greatly, although I did not come in as close contact with him as I should have liked to, was G. N. Lewis. He, as well, was a true scientific philosopher. This is well expressed in one of his lectures on "The Anatomy of Science":

The scientist is a practical man and his are practical aims. He does not seek the ultimate but the proximate. He does not speak of the last analysis but rather of the next approximation. His are not those beautiful structures so delicately designed that a single flaw may cause the collapse of the whole. The scientist builds slowly and with a gross but solid kind of masonry. If dissatisfied with any of his work, even if it be near the very foundations, he can replace that part without damage to the remainder. On the whole, he is satisfied with his work, for while science may never be wholly right, it certainly is never wholly wrong; and it seems to be improving from decade to decade.

Only a limited part of my time was spent in attending formal courses, which included also special training in mathematics and chem-

istry. The rest, I spent in the laboratory. In addition, I had to look for ways to earn some additional income. Bobili had made several unsuccessful attempts to obtain work as a librarian, for which she had been trained during her last year in New York. I was more fortunate, since I could always fall back on my old experience as a teacher. Upon the recommendation of one of the chemistry professors, I obtained a tutorship for a wealthy young man who had to learn the rudiments of chemistry in a very short time, so he could become president of a chemical concern. I condensed for him all the theoretical and practical aspects of inorganic and organic chemistry in the space of a few months. Further, my newly gained knowledge of biochemical analysis made it possible for me to help in the development of a project which was given to the laboratory and which involved the making of numerous analyses of human saliva. For this work I was paid, as an expert, sixty cents an hour, which was far better than the twenty cents an hour that I had been accustomed to earn at Rutgers.

These supplementary sources made it possible for us to manage our life quite comfortably. We could even go to San Francisco occasionally to a theater or a concert. When Peisi, having become tired of New York, soon joined us in Berkeley, we were happy with our own little circle, as well as with the many friendships that we were able to establish in a short time.

California itself offered us many natural beauties which we had never known before. The climate, the sea, the mountains, the whole environment, could have satisfied anyone. Even without a car and with limited resources, we were able to see a lot of the territory around us. We took many hikes in company with friends. The Berkeley hills offered inexhaustible sources of pleasure. As we would sit there watching the sun set over the Golden Gate, we would feel that nature could hardly be more beautiful, more compensating, more inviting.

Thus our days were full. Although the apartment was small, it was close to the university, and we could entertain a few friends. After a simple breakfast, I used to depart for the laboratory and for my classes. I would come back for lunch, which was followed by my tutoring and by my work on the research problem. The evening was usually spent in the library, in the laboratory, or with friends.

Thus we lived in an ideal environment, where one could pursue

a course of study and research without being unduly disturbed. Unfortunately, this peace of mind was marred by the great world catastrophes, the war and the Russian Revolution. When the United States entered the first World War in 1917, I had to face certain important questions. First, whether to join the Army in one capacity or another or to stay at the university and complete the course of training for my Ph.D degree. Second, what attitude to take to the revolution that was going on in the country of my birth. The first problem was decided quite simply, by following the advice of the Registration Board and good common sense, namely, to finish my work and obtain my degree, since I was so close to it. The second was more difficult, especially when the group of Russian students at the university, some fifty in number, including Peisi, decided to return to the country of their birth. This decision was taken before the October, or the Bolshevik, Revolution.

Dr. Robertson was uncertain of his own future. His wife was not happy in California, and he decided to accept a professorship at the University of Toronto. He urged me to try to finish my work in the middle of the second year, so that he could still examine me. The fellowship which I had held at the university during the first year was not renewed, because of unsettled conditions. I had to look, therefore, for a source of income elsewhere. My previous training and experience made it possible for me to obtain employment at the Cutter Laboratories, a company engaged in the manufacture of bacteriological products, such as antitoxins, vaccines, and serums. Beginning July 1, 1917, my time was divided between the commercial laboratory and the university laboratory. At the latter, I completed my investigations and prepared the material for my thesis. The various preliminary and language examinations took but little of my time and effort. Even the final examinations came off easily.

Most of my interest and energy were now centered upon the new experience, that of producing bacterial products for combating infections of man and animals. I was in charge of the biochemistry department. My job consisted in supervising a group of some ten or fifteen men and women who were preparing various media for the growth of different organisms. I had sufficient time left to devote to research on the development of a new type peptone required for the

production of diphtheria toxin. All the previous peptones used for this purpose had come from Germany, and our supply was cut off by the war. I thus contributed in a small way to the tremendous efforts going on in the country at large to become independent of Germany in the chemical and bacteriological fields. I was also called upon to develop special media required for the growth of new organisms that had made their appearance as a result of the war.

When all my work was completed at the university in December, 1917, the company offered me a full-time position, which I gladly accepted, especially since I had to wait for my degree until the coming May.

Before very long I had to decide what to do next. My interests were definitely not in the manufacture of biologicals. My mind was turning more and more to my first love, the soil microbes. I worked on the actinomycetes in what little time I could spare. I was longing to return to them as a major problem. The two years that I spent at the University of California and at the commercial company suggested to me new ideas, new approaches, and especially new tools for further study and for broadening the whole field of microbiology. If I could only have another opportunity! I would then proceed from the search of new organisms in the soil to a study of their activities, their importance in soil processes, and possibly learn how to control them and even how to utilize them further. I wanted to go back to a soil laboratory or to a general microbiological laboratory.

In the spring of 1918, various offers of positions began to come in. These ranged from teaching biochemistry in a medical school to investigating the chemistry of plant products at an experiment station. The most tempting, although the least lucrative, came from my old teacher, Dr. Lipman, to return to New Jersey and take over the work in soil bacteriology.

During my stay in California, I did not lose contact with the soil and with its microbiological population. First of all, Dr. Lipman's brother, Professor Charles Lipman, held the chair of soil bacteriology at the University of California. Although I was rather critical of his methods for studying the microbiological processes in the soil, I saw him frequently, since he was also a Rutgers graduate and our early training had been laid out along similar paths. He had only a very

limited knowledge of the soil population, which to him, as to many others, comprised only the bacteria. I argued often with him. His usual reply was somewhat as follows: "These subjects are hardly worthy to be designated as sciences! How many soil bacteriologists do you have in the National Academy of Sciences, and, for that matter, how many soil investigators?" My reply was that this was not the fault of the sciences themselves, but of the scientists. He would then call me a dreamer, and let it go at that. When, some twenty years later, I was elected to the National Academy as a soil microbiologist, I felt more proud of the science that I represented than of my own contributions to it.

Still, Charles Lipman held an important position at the university; he had some capable collaborators, and I could not refuse his invitation to attend their weekly seminars. This gave me an opportunity to meet frequently the other members of the Soil and Plant Divisions, notably Dr. D. R. Hoagland, who had made some highly important contributions to our knowledge of plant nutrition.

Not all my experiences in California centered at the university or in industrial research. During the summer of 1917, when I was entitled to some three months' vacation from university work, I decided to spend a few weeks working at "El Verano," a ranch near Sacramento. My duties consisted in taking charge of an irrigation system on a large agricultural area, in which beans, peas, and other truck crops were grown. Here I came in contact with a new type of American, the so-called "hobo," or transient laborer. I lived with some two hundred men in a general dormitory, ate with them, and spent most of my spare time with them. I would listen to them in the evenings and on Sundays and try to penetrate into their lives, their lost hopes, and their aspirations. I very much regretted that my knowledge of the English language, especially its idiomatic structure, was not yet perfect and that I lacked the pictorial expressions necessary to record the stories of these men's lives. Most of the men were simple people, whose lives had been broken for one reason or another, and who found satisfaction in "taking to the road." Many of them spent only a few days at the ranch. They saved a few dollars, then went on. During that summer at this ranch I learned to appreciate a type of human mind that I had not believed could exist in the United States, namely, the

"oppressed" type, which I knew so well from Russian literature. Here the works of Dostoevski, Gorki, and other great Russian novelists, who depicted so vividly these degraded lives, came back to me in new forms and in new expressions. But my path did not lie along the road to literature. Having saved up a few dollars and having learned a new type of farming, I returned to Berkeley, ready for my next year's work.

The two years spent in California thus contributed in many ways to my scientific knowledge as a whole, and to certain disciplines in particular, notably in biochemistry, organic chemistry, and physical chemistry—all of which were to serve me later in the attainment of my purpose of finding the role of microbes in the cycle of life, and finally a better understanding of human nature itself.

My decision to leave California and to return East was based upon several considerations. First and foremost was the opportunity offered me to return to a field of work for which I had been preparing all these years. Second, my old teacher, Dr. Jacob Lipman, had enough confidence in me not only to place under my charge an important phase of research in his own department, to which he himself had devoted most of his scientific years, but to permit me to change the title of this subdepartment from "Soil Bacteriology" to "Soil Microbiology." Third, the mild climate of California did not fully agree with me. I had been born and brought up under vigorous climatic conditions. During the two winters that we spent in Berkeley, I kept longing for the deep snows and the heavy frosts. The final contributing factor was the opportunity to live near New York, which was, after all, the great intellectual center of America and close to other centers of research like Washington and Philadelphia.

For these reasons, I accepted the offer from Rutgers and returned there, after an absence of exactly two years. The official title given me was that of Lecturer in Soil Microbiology at the college and Microbiologist of the station. This designation, which with the recent development of the field of antibiotics has since become so popular, was then given to few scientists in America. The reason I asked for this particular title was very simple: not the bacteria, but the fungi and the actinomycetes formed my major interests among the microorganisms. The broader description suited my present and future field of study much better than the narrower one.

In spite of my title and the two appointments, my salary was only fifteen hundred dollars a year, far less than I received in California. It meant a return to a life of considerable modesty, if not outright poverty.

My decision to return East brought up another immediate problem: where to get the necessary funds for travel. The two years in California had left me no richer than when I reached that friendly state, in spite of the fact that the last six months at the company had brought me a more substantial income than I had previously been accustomed to. I was on the point of asking Dr. Jacob Lipman to send me an advance of a month's salary, when his brother Charles, to whom I disclosed my predicament, offered to loan me a small sum for the railway tickets and for meals on the way. I returned this sum to him gratefully less than a year later, as I did the funds borrowed for going to California.

Thus came to an end our stay in California. It was a pleasant interlude. It was an enriching experience. We look back to it as two of the pleasantest years in our lives.

My Position at Rutgers

IT WAS a gloomy day that July 1, 1918, when I returned to New Brunswick. The laboratories of the Experiment Station were half empty. Many members had joined the Army or one of the war services, in one capacity or another. The microbiological laboratory was bare. The tables in the laboratory were covered with dirty dishes. The drawers were full of dried-up residues of various organisms. Nearly all the cultures of fungi and actinomycetes that I had left only two years previously in such fine condition were either dead or required careful manipulation to rejuvenate them. There were no graduate students left. There were no assistants, scientific or technical.

My first task was to clean up the place, hire an assistant, and start some research project. I also began to plan a course in soil microbiology which I would offer to some prospective students. Since the country was in the midst of the first World War and since the available funds for research work in the department were very limited, one could never be certain what the next day might bring. All arrangements had to be made largely on a makeshift basis. Dr. Halsted had retired and shortly afterward passed away. A middle-aged woman, who had been helping him with certain routine plant measurements but who knew nothing about a chemical or bacteriological laboratory and who could not make up media or use an analytical balance, was transferred to me as a technical assistant. To help me with some of my scientific work, a recent graduate of Rutgers was also assigned to me.

After going over with Dr. Lipman prospective research projects, I decided to devote my time to the completion of my earlier studies

of the soil actinomycetes. This problem, begun so propitiously in 1915, had been continued in New Brunswick by Curtis, and by myself in California. It was merely a matter of growing these organisms on a number of media and of measuring certain cultural and biochemical properties of those cultures which we designated as type organisms, so as to fix their identity for future reference. Dr. Lipman asked me to look into another problem that interested him at that time. It dealt with the possible role of fungi or bacteria in the oxidation of sulfur in composts. I was to attempt to isolate the organisms, if this was actually a biological process; and if one or more organisms were involved, I was to determine which was responsible for the process.

But before I could make final plans for living in New Brunswick I had to face another difficult problem, our precarious economic position. My new salary at the Experiment Station for full time was hardly enough for a married couple, with a child soon to come. Living costs were extremely inflated. We had nothing to fall back on, except hope and energy. But this was not sufficient. It looked as if I would have to look again for some supplementary work to increase our income. We had no furniture and little clothing. I had been looking forward to the day when, after receiving my degree and a position, I could settle down, free from worry and need for earning additional income, to a full-time research and teaching program, for which I had been preparing all these years. But this was not yet to be. Economic difficulties now forced me to consider favorably various opportunities in industrial laboratories. A number of openings were available and I was being urged to accept one.

Our country, which had been so dependent on Germany for its supply of chemicals, glassware, and even scientific literature, was gradually freeing itself of this yoke. This opened tremendous possibilities for trained people, especially those who had a proper chemical and bacteriological background. I now had such training and received, therefore, many attractive offers. Out of these, I selected an appointment at the Takamine Laboratory in Clifton, New Jersey. The work involved the study of toxicity of certain drugs (salvarsan) used for combating microbial infections of man (syphilis) and the study of the production of diastatic and proteolytic enzymes by fungi, a problem in which I had become interested in California. This appeared to be

most suitable to my position and interests, not only because of the larger salary, which was three times greater than that which I received at Rutgers, and the attractive type of work, but also because this company was close enough to New Brunswick so I could continue some research work at Rutgers. Unfortunately, I soon discovered that this was physically impossible, since each position required intensive concentration and preparation. It was decided, therefore, that I should devote five days a week to the industrial laboratory and only one day a week to my position at Rutgers. It was thus barely possible for me to continue my studies of the actinomycetes and give one lecture a week to a small group of graduate students.

To avoid excessive travel, we decided to move to New York City. From here, I could go directly either to Clifton or to New Brunswick. It was not an easy program, attempting to handle two rather difficult positions. At Takamine, I had to check every batch of the drug salvarsan which was being produced by a group of chemists. The success of the company depended upon my efforts. I also had to spend considerable time in learning new techniques of handling fungi on a large scale, and in trying to develop new methods for the production of fungus enzymes which could find industrial applications. I was surrounded mostly by a group of Japanese chemists, who did not trust me too much and who apparently were always afraid that I would run off with some of their secrets and sell them to the highest bidder. But Dr. Jokickhi Takamine himself was both a gentleman and a scholar. Unfortunately, he rarely came to the laboratory, and he was insufficiently familiar with the latest ideas and techniques in drug control or in the research problem entrusted to me.

The day that I spent at New Brunswick passed mostly in consultation with my new assistant and with several graduate students. I also had to check with a part-time secretary the material that I brought in after many evenings of work at home during the preceding week. This work consisted in correlating and co-ordinating numerous data which were to serve as a basis for a proposed monographic treatment of the actinomycetes. I also had to prepare the material for the weekly lecture in soil microbiology. There was not much time left for me to attempt to give a course in the laboratory. Even my lectures were not properly organized, since there was no suitable textbook and I

was still groping in the dark as regards the nature of the course I would like to offer.

Our economic position had improved considerably. Unfortunately, I could not say as much for my scientific work. The present arrangement could, at best, be only temporary. I would have to decide, sooner or later, to give up one or the other position. Dr. Lipman advised me to have patience and wait for the opportunity when a better salary could be offered me at Rutgers so that I could be reappointed there full time. I decided to follow his advice.

Thus began a trying period that was to last for a little over two years. Physically, it was a difficult situation, since it involved wasting three to four hours every day in travel. Scientifically, it was also difficult; many problems presented themselves in both laboratories, and I could hardly attack them with all the vigor of which I was capable, in the limited time at my disposal and with a complete lack of properly trained assistants or technicians.

After a year had passed, however, my course in soil microbiology began to take shape. The many nights spent in the preparation of my lectures began to yield results. To be sure, it involved extensive reading. This I should have enjoyed had not my time been so limited. The best I could do was to take the students into my confidence and tell them frankly that I disagreed heartily with the published facts and ideas and the available interpretations, but that I could offer them very little of anything else. If they were willing to bear with me, we would gradually analyze the accumulated facts, and in time perhaps come to some definite conclusions different from those in vogue. Most of the students were not happy about this attitude. As typical students, they preferred to have the reading matter digested for them and to have the prescription ready for them. I could offer nothing of the kind; I could not be expected to present to them information which I no longer believed to be correct.

In my research program at Rutgers, I could attend to only one project well and give some attention to one or two others. Naturally, I concentrated on my studies of the cultural properties of the actinomycetes that we had previously isolated. Even this progress was scarcely satisfactory, since I could devote so little time to the laboratory. I was now more anxious to get started with some important biochemi-

cal investigations than to limit myself to simple characterization and description of the organisms. I tried to give some attention to the sulfur problem assigned to me by Dr. Lipman, but progress was slow. It was very intriguing, but for lack of time it had to be limited to purely superficial examinations of various cultures until greater effort could be put on it.

At the Takamine Laboratory, with more time available for my work and better trained assistants, the results were more satisfying. My biological work on the toxicity of salvarsan was eminently satisfactory. I was learning for the first time the method of testing the effects of drugs on the animal body and was able to get considerable insight into the nature and activity of a classical chemotherapeutic agent. This suggested the possibility of finding other agents which would have broader antimicrobial activity. My work on the proteolytic enzymes was also making progress and gave me a broader insight into the practical approach to the study of mold metabolism. Unfortunately, the environment here was so unsatisfactory that I felt constantly that this was only a temporary position and sooner or later I would have to look elsewhere. Only seldom would one of the workers be freer than others to speak his mind. This was true, for example, of a chemist, Jokichi Oshima, who had a broad knowledge of the diastase-producing properties of molds and who taught me much of the potentialities of these organisms in food production, brewing, and in various industries. He had considerable practical and theoretical information on the utilization of some of the molds which form the basis of a very important industry in Japan. It was one of these molds that had been brought over to the United States by Dr. Takamine about 1890 and which served as a basis for the manufacture of the enzyme takadiastase. Oshima gave me some understanding of the Oriental mind. He was a sincere friend. But he knew little of Western civilization and was still young and somewhat inexperienced.

While my problems in the laboratory were thus gradually crystallizing in spite of the many difficulties, our life in New York offered many sources for satisfying intellectual curiosity. It made possible ready and free contact with scientific workers in the various institutions, including foremost, so far as my own interests were concerned, several members of the Rockefeller Institute. Among the many bril-

liant investigators, Dr. Jacques Loeb attracted me most. I admired his genial personality, his profound scientific approach to biological problems, his liberal philosophy. Robertson introduced me to Loeb by message and thus made it possible for me to pay frequent visits to the laboratory of this brilliant "mechanistic" philosopher. Loeb asked me to come to see him whenever I wished. I used to sit before him for hours and talk of my scientific interests and problems. He usually listened with fatherly attention and, in turn, suggested and discussed methods of approach to biological investigations, scientific aims, and scientific philosophy. He helped me greatly spiritually, by giving me freely his wise counsel, and practically, by pointing to potential errors of fact and of judgment. He thus helped me to weather a most critical period in my scientific career.

I also took advantage of various scientific meetings in New York and vicinity. I was particularly interested in the annual meetings of the Society of American Bacteriologists and in the monthly local meetings of the Society for Experimental Biology and Medicine. I was thus able to meet colleagues from various parts of the country who were interested in the same problems as I. Some of these chance acquaintanceships grew into friendships.

Frequently, when I went to meetings held in other cities, I visited investigators in my own and in allied fields, to discuss various scientific problems with them. I need refer here to only two such visits, to emphasize how much I learned from contact with outstanding investigators in various fields of science.

As I was about to finish my large paper on actinomycetes in 1919, word came to me that Dr. Roland Thaxter of Harvard University, a leader in the field of cryptogamic botany in America, who first described in 1891 a form of an actinomyces as the causative agent of potato scab, was highly critical of my work. In these studies, I attempted to characterize and describe new species on the basis of cultural and physiological properties rather than of differences in structure, which could not be distinguished at that time. At the first opportunity, when the bacteriologists met in Boston in the winter of 1919, I visited Dr. Thaxter and spent an afternoon with him. He was a profound thinker and a sincere scientific worker. He listened attentively to my arguments defending my method of approach to the study

of a little-known group of microorganisms. I believe I succeeded in convincing him that we were dealing here with forms which showed relatively few stable structural properties; we must, therefore, rely for specific identification largely, for the time being at least, upon their growth characteristics on different media. I had to agree, however, that fundamentally he was right. I learned a great deal from that interview. We parted good friends.

I paid a similar visit three or four years later to the eminent protein chemist Dr. T. B. Osborne of Yale University. I came to discuss with him the role of plant proteins in the decomposition of organic residues in the soil and in the formation of humus. It was Dr. Osborne's opinion that plant proteins are resistant to decomposition and accumulate in the soil. He based this opinion upon the fact that complex plant material, such as straw, does not yield ammonia upon decomposition, whereas animal materials, such as dried blood or tankage, yield great quantities of ammonia when added to the soil. I convinced him that the question was not one of the nature of the protein, but rather of its concentration or the ratio of carbohydrates to proteins. This ratio was much wider in such plant residues as straw, with the result that there was a depression in ammonia accumulation rather than in ammonia formation. Here, as well, I gained a good friend, who promised to help me by supplying me with samples of pure proteins and by advising me in connection with a most difficult problem that I was now facing, the unraveling of the origin and nature of humus.

As often as I could, I visited Washington, with its numerous government laboratories. Here I renewed and extended the friendship that I had established, under such opportune circumstances in 1915, with the mycologist Dr. Charles Thom. Perhaps of all those with whom my good fortune brought me in scientific contact, aside from my own students and some of my former teachers, he has been throughout my scientific career, especially in the most difficult early days, my most devoted friend. He helped me greatly in laying the basis for my future work on the fungi and actinomycetes. He advised me wisely when I sought his advice and has remained a close and devoted friend. He recognized my early profound interest in the microbiological population of the soil, my groping for ideas and for facts, and was both

sympathetic and willing to help. When upon the publication of my first note on soil fungi in 1916, it was subjected to a severe attack by Dr. H. J. Conn of Geneva, New York, who found fault with my methods and criticized my results, Thom came at once to my rescue. He wrote to me not to be disturbed by such criticisms, that I was on the right path. He emphasized that only more detailed studies, and not arguments, would prove the truth. He counseled me to wait, accumulate more information, and, when the time came, reply to criticisms with further proof. This advice I carefully followed.

Thus the period of 1918–1920 passed. I was gaining considerably in knowledge and experience, in the philosophical approach to the science, and in the practical application of the results. It was a difficult period, involving much work and much “soul searching.” But it served gradually to establish and later to consolidate my position in the scientific world.

This period also produced certain important changes in my personal life. On September 15, 1919, Byron was born. The birth of the child meant renting and furnishing an apartment. These costs, combined with the various doctor and hospital bills, kept our finances at a constant low, in spite of the fact that I was now earning a fair salary. We managed, however, to pay off our debts gradually and even saved up a few hundred dollars for a future emergency. Bobili now had an opportunity to go more frequently to concerts and to the theater, which she loved so much. I was only too happy to stay home and read so I could organize my lectures properly and get a better grasp of my field of science.

The international situation was perhaps the one element that proved most disturbing during that period. The war in Europe came to an end with victory for the Allies. But the country of my birth went through a series of cataclysmic revolutions. These were followed by civil war, which shook the country, and nearly the whole world, to its very foundations. This was true particularly of my native Ukraine. Within a period of a year or so, that region passed through several governments, some of which were nothing but groups of local bandits, who took advantage of the disturbed conditions to rob and murder the local population. Vinnitsa, the city of my father's birth,

went through the hands of seven governments. The town of Priluka, though in itself of no political significance, was also subjected to considerable destruction.

My father, his new wife, and the two children born to him in the meantime, all died during the epidemic which followed the war. Bobili's parents and four brothers survived, although one of the younger brothers was badly wounded by a local bandit, and so was her mother in trying to protect him. They were suffering from lack of food and clothing. There was nothing that we could do to alter their situation radically. We helped in every way we could with parcels. When conditions quieted down and a stable government was established, normal correspondence was resumed. All our efforts to bring them to this country failed. They steadfastly refused to leave their homeland.

On January 1, 1921, I returned to full-time duty at the New Jersey Agricultural Experiment Station and at Rutgers College. Although the new salary was still small, it sufficed to allow me to engage fully in my scientific work and in teaching. I could forget for the time being all worries that had been harassing me for many years about earning extra funds to meet our modest needs. We moved back to New Brunswick, rented a small house, and settled down to the quiet family life of a young college instructor. I was not given a full college appointment until five years later, when I was made Associate Professor.

My first task was to reorganize the microbiological laboratory and to start some new research projects. In this, I had the encouragement and full support of Director Lipman, who was official head of the department, as well as of the authorities in Washington who supervised the Federal funds that were assigned to me for my work. I had been anxiously waiting for the time when I could begin. But before I could attempt to do that, I had to complete the two investigations begun previously. These were the metabolic studies of the actinomycetes and the isolation and study of the physiology of the organism concerned in the oxidation of sulfur in soils and in composts.

Some of the data on the actinomycetes were presented in several scientific papers. The large number of organisms and the complex problems involved in the study of the physiology of these organisms made it evident that one could hardly expect a successful conclusion of these investigations because of lack of methods and ideas. They

were, therefore, allowed to remain dormant until new approaches could be developed.

The sulfur problem proceeded at a more rapid rate. Within six months after my return to the laboratory, my assistant and I succeeded in isolating the organism concerned in the oxidation of the sulfur. We named it *Thiobacillus thiooxidans*. It proved to be a highly interesting organism, especially from a physiological point of view. Its isolation attracted considerable attention. As I look back, I believe that, before the subject of antibiotics came to the front in our laboratory in 1940, this may have been the most important scientific contribution that came from this laboratory. In this I was assisted first by Jacob Joffe, who devoted much time and energy to this problem, and later by a new assistant, who gradually became my lifelong associate and friend, Robert L. Starkey. The results of these studies were published in a series of scientific papers in various journals.

Meanwhile, a number of graduate students began to arrive at the laboratory, primarily to work in soil microbiology. Rather than assign each of them a separate problem and let the laboratory become a bewildering mosaic of ill-fitting investigations, I used these students largely in the solution of the major study which I had undertaken. I could thus carry out my work on a much broader and often more profound scale than the modest funds allowed me by the institution would otherwise have permitted. Assisted each year by two to six young investigators who came to work for their Ph.D. degrees or to spend varying periods in the laboratory, I plunged at once into two problems that were to continue for many years. These were the evaluation of the methods used in soil microbiology and the study of decomposition of organic residues in soils and in composts leading to the formation of humus. These two studies could never have received sufficient consideration nor could they have been brought to a successful conclusion without the help, collaboration, and frequently most excellent contributions of these assistants, students, and guests. I may truthfully say that without them a large part of my work would have remained limited in scope and short of execution.

These students helped me also in another respect, by virtually forcing me to devote a considerable part of my time to the preparation of a comprehensive series of lectures in my selected field of science.

I was thus led to make a detailed survey of the microbiological population of the soil, of the numerous activities of these organisms, and of their role in soil processes. The lectures, after being repeated three or four years in succession, formed the basis for my treatise *Principles of Soil Microbiology*, the first edition of which was published in 1927 and the second in 1932.

Meanwhile, the scientific contributions coming out of my laboratory began to attract attention in this country and abroad. They led to numerous inquiries and to an extensive correspondence with scientific workers throughout the world. Most of these letters were friendly and constructive and resulted in a better appreciation of microbiology and especially of the relation of microorganisms to soil processes. Occasionally, however, I had to meet criticisms and attacks by other scientists, who either disagreed with my methods or with my interpretations. In some cases these exchanges of letters ended in a friendly manner, which contributed to the advance of the science. A few, fortunately very few, cases did not end so happily. I have never been afraid of a fight, but it has to be on the level: I have never been willing to stoop to underhanded innuendoes and personal squabbles.

One of the most comprehensive experiments that I undertook upon my return to the laboratory for full time was based upon the need for throwing further light on the relationships between protozoa and bacteria in the soil, and the role of protozoa in soil fertility. This was the problem that had been assigned to me in 1915, when I first began graduate work, but which was soon abandoned because of my insufficient training and knowledge of methods and approaches. Now, assisted by Starkey, I began to investigate the effect of partial sterilization of soil, by the use of heat and antiseptics, upon the abundance and activities of various groups of microorganisms and their relation to one another. Particular attention was paid to the effects of the protozoa upon the bacteria, not leaving out of consideration the fungi and actinomycetes.

Our results pointed to a totally different picture from that painted ten or twelve years before by Russell and Hutchinson at the Rothamsted Station in England. We could not support their claim that protozoa affect injuriously the fertility of the soil by devouring the bacteria. On the contrary, our results brought out the fact that in a normal soil there exists an equilibrium between the different members of its micro-

biological population. Any disturbance of such an equilibrium, which is brought about by treating the soil with steam or dry heat, with chemical antiseptics, or even by mere drying and wetting, will result in a series of changes in the nature and abundance of the different types of organisms which make up this population. These changes in the microbiological make-up of the soil could explain the resulting biochemical changes and the improved fertility far better than the mere assumption of existence of only protozoa and bacteria.

Russell's concept, which came to be known as "the protozoan theory of soil fertility," was reminiscent of the old idea of God and the Devil, Good and Bad, Light and Dark. Here were the bacteria, the friends of man, or the "good microbes," and here were the protozoa, the enemies of the bacteria, the "bad microbes." According to this theory, bacteria are responsible for soil fertility; the protozoa destroy the bacteria and thus keep down fertility. When the protozoa are killed by heat or by antiseptics, the bacteria, freed from their natural enemies, begin to multiply rapidly and thus bring about increased soil fertility.

This sounded interesting, almost melodramatic. It had an appeal. Our studies were far more prosaic. They demonstrated that the fungi and actinomycetes were completely disregarded in Russell's theory. These organisms have highly important functions in soil fertility, far more so than do the protozoa. By omitting them from consideration, the whole "protozoan theory" lost contact with facts.

Our results led to a friendly interchange of letters with Sir John Russell of Rothamsted and later with several of his collaborators. At the suggestion of Dr. Russell, the Rockefeller Foundation supported an exchange of investigators between our two laboratories, with the result that one of my students went, in 1924, to spend a year at Rothamsted, and one from Rothamsted came to spend a year in our laboratory. These studies contributed to a better understanding of the soil microbiological population and brought about closer collaboration between the two laboratories.

Another friendly exchange of correspondence was begun in 1921 with Professor S. Winogradsky, the eminent Russian bacteriologist, who as a result of the revolution had left his native country and sought refuge at the Pasteur Institute in France. At the suggestion

of his former assistant, V. L. Omeliansky, another famous Russian bacteriologist, who remained in Russia, and with whom I established active correspondence immediately after the war, Winogradsky asked me to help bring him in contact with various American investigators, especially those interested in soil microbiological processes. This I gladly did, and thus gradually began an active correspondence which was to last for more than three decades and which was to lead to a deep friendship, rich in consequences.

Another illustration of a type of correspondence which led to friendly collaboration may be recorded here. As I have already mentioned, my first published results of methods of studying soil fungi and actinomycetes, in which I established not only that these organisms are found in large numbers in the soil but that they lead there a normal existence, brought down upon me severe criticism from Dr. Conn of the New York State Experiment Station. He argued that my claims could not be based upon factual evidence, since most of the fungi are probably dust forms and get into the soil by chance. They cannot, he said, be considered as leading a normal vegetative existence in the soil. He also criticized my work on the actinomycetes. Since this occurred while I was still in California, I was in no position to obtain immediate additional evidence to support my first experiments. But soon after my return to New Jersey, I began to carry out a series of studies which would tend to supply the necessary evidence. I submitted a paper in which I refuted the above criticisms. Meanwhile, Conn himself developed new techniques which enabled him to confirm rather than discredit my data. He offered to accept my interpretation. I at once withdrew my own paper, although it was already in proof form. At the invitation of Conn, I visited him in Geneva and we spent a whole day discussing various problems concerned with the study of soil microorganisms. Conn further suggested that a close collaboration between our two laboratories would result in building up a new school of soil microbiology in America. This suggestion was highly tempting. But my interests were more in the biochemical and analytical directions, whereas Conn's were in the systematics of bacteria. He could not possibly be of much help to me, or I to him.

Less fortunate were the results of my extensive correspondence with another soil bacteriologist, Dr. F. Löhnis, who was at that time

connected with the Department of Agriculture in Washington. He was a native of Germany, the author of two monumental books on agricultural bacteriology and of numerous scientific papers, and one of the editors of an important journal in which most of the developments in microbiology had found a haven before the first war. Löhnis was in the habit of submitting to highly critical, biting, and satirical attacks all works which expressed disagreement with his ideas. This brought him into trouble with some of his colleagues in Germany. In 1913, he took advantage of an invitation to come to America to undertake a study of the bacteriology of stable manures. Here he paid little attention to that problem and devoted himself largely to the study of the life cycles of bacteria and of bacterial variations. As an enemy alien, he had to live outside Washington during the war. Since apparently it was not easy for him to move his staff to a new laboratory, he limited himself largely to library work. He made important contributions to his subject, but his ideas were not universally accepted.

Löhnis was also one of the initiators and the major promoter of a method in soil microbiology which became known as the Remy-Löhnis solution method. In 1923, I published a paper on the significance of ammonia formation in soil and in artificial culture media as a method of evaluating the microbiological state of the soil. I tried to reinterpret the results obtained by this method, which was at that time in vogue in the various soil bacteriological laboratories here and abroad. In analyzing the results obtained by this method and comparing them with those obtained by enumeration of microorganisms in the soil, I could not help but recognize the inadequacy of such methods in evaluating soil fertility. I was thus inclined to take a critical attitude toward the claims of some of the earlier investigators in this field.

As soon as my paper appeared, Löhnis became greatly incensed and wrote a bitter attack upon my work, actually accusing me of arranging my data in such a way as "to show him up"; he emphasized that if my data were only arranged otherwise, "Waksman's assumptions would fall to pieces." His paper was submitted to *Soil Science*, of which Dr. Lipman was editor. It was shown to me for my own comments. I immediately wrote a reply, not only justifying my claims but extending them further. When his paper and my reply were returned to him by the editor, Löhnis insisted that he would

have the last word and requested permission to reply to my reply. Otherwise, he threatened, he would be forced to do it in some other journal. Dr. Lipman advised me to go to Washington to see Löhnis and settle the controversy with him personally, so as to keep this from becoming a bitter polemic. I found him a rather charming companion and after a few hours' discussion succeeded in convincing him that I was right. He offered to withdraw even his original paper and thus stop the potential controversy. I breathed a sigh of relief.

Unfortunately, this was not the end of my relations with Löhnis. He was an embittered person and, although pleasant in personal contacts, felt enraged whenever his work was adversely criticized. When Winogradsky, in his turn, criticized his method, he attacked Winogradsky viciously, going to the extreme of accusing him of trying to please his French hosts by criticizing a German professor.

In 1925, Löhnis returned to Germany, without a good word for American hospitality. When my book *Principles of Soil Microbiology* appeared soon afterward, he published an extensive review, tearing my book to pieces. He carefully combed the thousand-page volume for minor errors and doubtful statements. The first edition of a comprehensive volume like mine was bound to contain certain errors. He finished the review with a vainglorious attack upon the United States, to the effect that "America has now become a rich country and much could be expected of it, but when such books appear, one becomes completely discouraged." When this bitter attack appeared, I felt unhappy and was on the point of writing a reply, but my friends in Germany advised against it, since those who knew Löhnis would conclude that my book must be good if it called forth that type of vituperation. Other German bacteriologists considered the advisability of publishing a German translation of my book; this fell through because of the size of the volume and the fact that many English copies had already found their way into Germany. Löhnis's wife must have felt the injustice of his attack on me, for upon his death some years later, she sent me his photograph and his personal annotated copy of his own book.

The three and a half years of 1921-1924 were characterized by great progress in my work and the release of many scientific papers from my laboratory. My knowledge of Russian, German, and French

proved to be of great help, for it facilitated not only intimate knowledge of the work of foreign investigators but also extensive correspondence and later personal contact, which led to many close friendships.

Our private life continued as before. Byron grew rapidly and promised to become a normal boy. He was soon ready for school. Unfortunately, when he was four years old, he developed mastoiditis in his left ear. This caused us considerable worry and involved the expenditure of a large part of our savings. We then fully recognized the great difficulties that a person of the lower middle class experiences when a member of his family becomes seriously ill. The doctor who was to perform the necessary operation and postoperational care requested a sum of money that would have amounted to a little over a half of my annual salary for that year. Fortunately, another doctor recommended to us performed the operation in a very satisfactory manner for only a modest compensation. The child had to be taken to the doctor in New York several times a week for a period of a few months. This involved further expenditures. But the boy got well rapidly, although his hearing in one ear remained permanently affected.

These extra expenses, as well as the help that we were rendering Bobili's parents, again forced upon me the same old problem, a search for some additional income. This I tried to obtain in several ways. My knowledge of Russian proved to be highly helpful at that time. The soil chemists in Washington had become interested in the work of Gedroiz, a brilliant Russian investigator, and asked me to undertake a systematic translation of all his papers. I was well paid for this, sufficient to rebuild our savings account. This work also proved helpful in enabling me to refresh my knowledge of the Russian language and enrich my scientific vocabulary.

I found a second source of income, even more remunerative, by becoming a consultant for industrial organizations interested in the manufacture of enzymes by fungi. My theoretical work in California and my practical studies at Takamine's had given me a broad basis for an understanding of the problems involved in the production, utilization, and mode of action of microbial enzymes. I collected an extensive literature on the subject, which served as a basis for a review paper and later for a book, *Enzymes*. In the preparation of this, my first book,

I collaborated with Dr. W. C. Davison of Johns Hopkins University.

As a result of my special knowledge of enzymes in general and of microbial enzymes in particular, I was often called upon to assist in the solution of various problems by enzyme manufacturers. Since my work at the Experiment Station was not concerned with this particular subject, Dr. Lipman felt that I was free to advise such companies and make suitable financial arrangements, provided I did the work in my spare time. This supplementary income made possible various extra expenditures which went to make life fuller and pleasanter.

Throughout my scientific career, then and later, I was forced constantly to fall back upon industrial connections to help me meet the modest financial obligations of my small family and make possible my scientific journeys to Europe. These contacts did not consume much of my working time, and the compensation received was far greater than could have been measured in terms of the extra dollars. These connections made me realize that very frequently scientific investigations which do not appear to have immediate practical applications may lead in time to important developments, and that an investigator searching for scientific principles need not discard any practical results that come his way. This experience served me in good stead later, when I undertook the study of antibiotics. Had I not appreciated the practical potentialities of these compounds, their study in our laboratory would undoubtedly have resulted in certain purely theoretical records and might have led to no important practical developments until taken up later by industrial laboratories.

I was always careful, however, of the use that was made of my information. Had I the least suspicion that it would be misused, I immediately refused to supply it. The following illustration will suffice. Soon after my return to New Jersey for full time, I was asked by an old friend, whom I had known since my early graduate days at Rutgers, to help him analyze a vitamin preparation which a group of promoters was planning to place on the market. The subject of vitamins was just becoming very popular, and these promoters were anxious to reap some of the benefits. My friend had no idea how to test for vitamins and asked me to help him. In doing so, I submitted to similar tests an enzymatic soybean digest which I had worked out in the Takamine laboratory. My own preparation was extremely active,

whereas that of the promoters had barely any activity. I reported this to my friend. This was immediately followed by an offer, in a luxurious office in a New York skyscraper, of a fantastic sum of money for my formula, provided I would approve the use to be made of it, namely, extensive publicity and exaggerated claims. I refused the offer without further consideration.

Europe Revisited . . . Grand Scientific Tour

TOWARD the latter part of 1923, it became evident to me that my career in soil microbiology was coming to a crossroad. I had now become quite familiar with the various approaches to this subject. I had surveyed many of the widely accepted procedures and found them wanting. I recognized that the microbiology of the soil represents a broad field of science, a borderline science, to be sure, which involves not only microbiology but also soil processes and plant nutrition. There was no question in my mind concerning the role of microorganisms in soil processes, but there was a certain question that was continuously arising as to whether I was headed in the right direction. I had lost all respect for some of the procedures and ideas that had dominated the field of soil microbiology when I was still a graduate student, but there were no others to take their place. I was groping for new ideas and methods. I might almost have compared myself to a fountain that required new sources, otherwise it would dry up.

It was becoming more and more evident to me that I should discuss, with the masters in the subject, the problems that were disturbing me. I must, therefore, look for an opportunity to visit the various schools and scientific centers in Europe, which was still the fountain-head for most sciences. I must meet the outstanding men in the field, talk to them, and obtain their reactions and advice.

Certain definite problems, which previously appeared only vaguely

on the horizon, now began to take clearer shape in my mind. I must attack them sooner or later. They were concerned with the decomposition of plant and animal residues in the soil and in the compost, and with the formation of humus. I was also beginning to think in terms of interrelationships among the members of the soil microbiological population and the effect of one organism upon another. I began to think more and more of the urgent need for a comprehensive treatise on the subject of soil microbiology, where the available knowledge would be brought together in a critical form. The time had come for me to make the grand scientific tour of Europe. It was important not only to visit the various laboratories in different countries, but also to discuss the problems, analyze current ideas, and determine where my own field of science, as well as related subjects, such as soil science and microbiology, was headed.

With all these ideas floating through my mind, I applied to the dean of the college for a six-month leave of absence. The fact that an International Conference on Soil Science was to take place in May, 1924, in Rome, gave me an additional excuse for asking for the leave. It presented an ideal opportunity of meeting many investigators in my selected science and thus establishing contacts with workers in the different countries. Dr. Lipman agreed that I should undertake the journey and granted my application. I was allowed my full salary, but no additional funds. We used all the limited savings (about a thousand dollars) that we possessed to buy the ship tickets and had to depend for the remaining expenses upon the meager monthly salary payments which were to be made by the college to my bank account.

We were to sail for Europe on April 9, 1924, landing at Plymouth, England. Our first visit was to be to Rothamsted, the oldest experiment station in the world and the home of some of the current ideas on soil microbiology. From there we would proceed to France and Italy. On the way, we were to stop in Switzerland and leave Byron with a private family at a small village in the neighborhood of Lausanne. From Italy, we planned to go, via Switzerland, to Germany and Russia. We were particularly anxious to visit our birthplace, see Bobili's parents and brothers, especially Peisi, and learn at first hand what the great revolution had accomplished. From there, we would return west via Sweden, Denmark, Holland, and France, and finally back home. It

was an ambitious undertaking, especially with the very limited funds at our disposal. But we did it.

Dr. Lipman, who was to serve as president of the Soil Conference, accompanied by Mrs. Lipman and Prof. Willard C. Thompson of the Poultry Department, were to be on the same boat with us, on our way to Europe.

I can best describe our journey and some of the general European impressions by quoting from my diary. This I kept faithfully, although there were frequently irregular intervals between the entries, due to interruptions by travel conditions. This diary began on the day we left for Europe. Numerous omissions have been made, since they represent only minor details.

April 9. The day promises to be clear. I feel tired and somewhat enervated. We arrived at the pier earlier than necessary, a common habit of mine. Some of our friends in New York came to say good-by to us and brought us some flowers. Dr. Lipman looks healthy and vigorous as usual. Bobili, who has been critical of our potential accommodations and a bit afraid of the trip, began to feel that perhaps this particular boat is not the worst that we could be on. We sail at noon, bidding a last farewell to our friends and to the assembled crowds. The sky line of New York is magnificent. Soon we pass the Statue of Liberty, with her raised torch, inviting those in need of liberty to come and repose under her protecting arm. A cool breeze is beginning to blow. The dining room is crowded. I feel fine, with only a slight headache. It has begun to rain and the boat is shaking slightly.

April 11. I had a long walk with Dr. Lipman. We talked of soil microbiology, its aims and prospects. He believes that future developments in the field lie in a more intensive study of the constancy of microbial types in the soil, the influence of environment upon such types, the colonization of microorganisms around plant roots and various soil constituents, and the influence of the composition of the soil solution upon the distribution of microorganisms. In other words, he is thinking of microbial ecology. If he had an opportunity to go back to scientific work, he would take up the study of the mechanism of nitrogen fixation, a problem on which he worked some twenty years ago. Most of the passengers appear to be writing diaries. Future gen-

erations will have to digest an immense amount of information about the daily affairs of American passengers on board steamers in the year 1924.

April 13. The day is rather stormy; we are in the middle of the Atlantic; the broad waves make the boat go up and down. I felt uncomfortable, but not sick. I made an effort to go to the dining room and swallow some food. Dr. Lipman increases the length of his daily walks, now to about seven or eight miles a day. The evenings he spends playing bridge. I see no particular fascination in this way of spending time. I would rather watch the waves roll by. Byron has become adapted to the sea journey, although he feels rather dizzy at times. He is making quite a hit with the ladies, but talks too much, eats too little, and learns to imitate.

April 18. Arrived early in Plymouth. The rays of the rising sun reflect the green-covered hills of the British shore. The little islands and rocks projecting here and there add to the beauty of the landscape. The greenness of the hills and the battlements here and there are especially impressive. The narrow-gage line and the quaint cars and locomotive remind me of other European countries. As soon as the train pulled out of the station, we became spellbound by the architecture of the houses with their curious chimneys, the cleanliness of it all, the gardens, the neatly cultivated fields, the herds of brown cows, the frequent flocks of sheep, surrounded by enchanting scenery. This called forth ideas of repose and quietness. It gave me the feeling of going away from the world of worries and of constant search to a life of rest and peace. One could easily understand why England has produced so many great poets, why sentimentalism in literature originated in England, why the famous elegy of Gray, the "Deserted Village" of Goldsmith, and the *Sentimental Journey* of Sterne were written here. The rivulets and streamlets, the dales and valleys, called to them.

We left for Harpenden, a small town near London. Since I departed from Europe nearly fourteen years ago, I have not had the same feeling as I have had here, especially when we went out at night for a stroll on the commons. It is not the environment and the spirit which are present here and which may be lacking in an American town of similar size, but the arrangements of the streets, the homes, their ar-

chitecture, the social or rather communal life, the freedom of the spirit, the comparative lack of automobiles, the country lanes and byways—all speak of another world, not that of hustle and bustle, constant chasing for material interests, but that of life for life's sake and for enjoyment. The tall trees, the quiet mansions, the cleanliness are things for which the heart has been longing all these years.

I went early to the Rothamsted Laboratory. This being Easter Monday, I found the front door locked, and the place nearly deserted. I found a little house in the back of the laboratory. The first man whom I encountered there proved to be Mr. Gray, the former caretaker. He took me to the experimental plots and talked a lot about the founders of the institution, Lawes and Gilbert, whom he knew personally. His observations were full of interesting details of the work of the men who made this laboratory famous. He spoke of the controversy between the laboratory workers and the German chemist Liebig concerning the role of organic matter in plant nutrition, of the careful work and stern character of the Rothamsted bacteriologist Warington, who tried to isolate the nitrifying bacteria, of the discovery of Hellriegel and Wilfarth, on the role of root nodule bacteria in the fixation of nitrogen by leguminous plants, and its effect on the Rothamsted program. We visited the manor built in the Jacobean style of the seventeenth century, a magnificent structure. The park with its cool inviting walks, the porches, the pheasants, and the whole environment so lacking in America, is reminiscent of the life of the nobility in Russia.

Finally I located Sandon, an assistant in protozoology. He took me through the laboratory itself, showed his own work and that of others. Dr. Cutler, the protozoologist, came in later in the morning and invited me to bring my family to his house for tea that day. We talked a lot about soil biological processes. He appeared to be ready to modify greatly the position originally taken by Russell and Hutchinson concerning the importance of protozoa in the soil as the limiting factor to bacterial development.

Subsequent visits to the laboratory, the following days, gave me a clear picture of the various laboratory and field experiments carried out at this famous institution.

April 24. Spent a good part of the day at the home of Sir John

Russell. We discussed the outstanding problems in soil microbiology, of which he has a fairly clear conception. He believes that further investigations should be directed toward the elucidation of the nature of the soil population, its possible changes throughout the year under the influence of the environment, the effect of one group of organisms upon another. The idea that bacteria are the only members of the soil population should be abandoned entirely. The bearing of microbial activities upon the nitrogen cycle in the soil must be elucidated, since nitrogen is the controlling element in the growth of cultivated plants. Russell also appeared even willing to modify his earlier ideas of the injurious effect of protozoa on the bacterial population and thus on soil fertility. My general impression of the group was that these people are very serious, anxious to establish the facts that govern soil processes.

The Railway Hotel where we were stopping has submitted to us, at the end of our four-day stay, a bill with the most exorbitant charges. The meals served there were simply horrible, the same soup being served to us twice a day while we were there. We hardly touched it but were invariably charged for it. We were practically "skinned," without justice or appeal. They took unmerciful advantage of us, since we have not learned as yet to get our money's worth and we simply have to do it, if we are to go through with our trip.

April 28. Have returned to London, to find what is called typical weather, drizzle and rain. Bobili went to bed with a cold. We were very sorry to have to give up a proposed trip to Oxford, because of the bad weather. Visited numerous bookshops. If we only had the money, what fine books we could buy here! Also visited the British Museum, with its marvelous collections. What a pity that the time is so limited!

The funds that we budgeted for ourselves are beginning to show the effect of our travels. It is really very awkward to have to count every cent spent, not only on sight-seeing or entertainment, but on mere necessities, especially when you come to new places every few days. We must learn to save on meals and special purchases. It is difficult to save on hotel bills.

We left for France. The sun came out and the day promised to be nice, but the channel crossing was horrible. Within the one and a half hours that it took to cross, we were all miserably sick. The boat rocked back and forth, up and down, so that it turned all our insides

out. Upon arrival in Paris, we were met by another rain. As a result of an unwise recommendation by a fellow American we stopped at a rather expensive hotel. I tried to find another cheaper hotel, but my French was poor, the weather was miserable, and most of the hotels were full, so we stayed on. Under these conditions, the first impression that we had of Paris and the French was not too pleasing. Since our allowance was largely spent on the hotel bill, we had to feast largely on tomatoes, rolls, apples, and milk.

April 30. Our first visits were to the Left Bank, with its universities and institutes. What a magnificent place, with bookstores at every step! At the Institut Pasteur, I met Prof. Besredka, who was graduated from Odessa University in 1892 and came to Paris in 1896, to remain at the institute since then. He knew Pasteur during that scientist's last years. He spoke highly of Metchnikov, whose laboratory he is now occupying.

At the Institut Agronomique, I met Prof. Keyser, a pleasant old man, who does not seem to keep abreast of the modern ideas in soil microbiology, and Guittoneau, a much younger man, who seems to be better equipped scientifically and more alert. The chemist André and the geologist Cailleux complained that France is poor and does not do enough for science. Saw the beautiful old gardens in the back of Institut Agronomique, the home of the great masters of soil science, Boussingault and Müntz.

On subsequent visits to the Institut Pasteur, I met Prof. Fernbach, who has devoted his life to the study of enzymes and fermentations. The words "putrefaction" and "decay," so commonly used in microbiological literature, have no meaning to him at all. The application of the term "fermentation" to the decomposition of carbohydrates is also coming to have no meaning. Met Fernbach's assistant, Max Schoen, an intelligent microbiologist, a native of Russia, and Mazé, who is working on the bacteriology of milk and cheese and is interested in plant nutrition and legume inoculants. We spoke again of the limited means spent in France for scientific work. Soil science and plant nutrition are particularly neglected. After such a splendid past!

May 3. We spent a most pleasant day yesterday, visiting Winogradsky at Brie-Comte-Robert. A quaint old village. In a little side street, there is a small tablet on a door opening through a stone wall: "Institut

Pasteur." Both Prof. and Mrs. Winogradsky proved to be very charming people. They have suffered a great deal as a result of the Russian Revolution: he had to escape via Odessa, on a French warship; she had to travel in a horse-drawn wagon for a hundred and fifty miles from Proskurov in the Ukraine to the Austrian boundary. She still has a frightened look on her face. Her hair is silken white. She is rather small in stature and wears heavy peasant shoes. There is with them a young relative, Mlle. Nikitina, who studied to be a singer, but who suffered so much in the revolution that she cannot come to herself again; her head jerks quite often. She serves as general assistant to Winogradsky.

Went to Versailles, where we visited the commercial laboratories of G. Truffaut, who manufactures bacterial preparations. The laboratories are extensive, the gardens magnificent, and the two hundred-odd people are busily occupied. The impression we carried away of the scientific work was rather depressing. It appears doubtful whether much will come out of here to advance soil science. A lot of trouble could be saved if they had only been familiar with the studies of the American soil microbiologists, which are apparently not known here at all or are disregarded.

May 6. We left Paris for Lausanne. From there we proceeded to the village of Yens, where we were to leave Byron during the remaining part of our European journey. A private home was recommended to us by friends. The lower floor in the house is used for keeping chickens and pigs; there are also a workshop and two rooms for visitors. The upper floor is inhabited by the family. The old lady was a governess for fifteen years and speaks good English. The people are very friendly. Two or three families occupy the same house, each having its own garden and patch of land for greens, fruit trees, flowers, and chickens. It was hard to part from Byron, but it was out of the question to take him along further on the journey, since it was becoming too tiring for him. We spent the night in one of the cold rooms downstairs, covered with heavy pillows.

The weather improved as we left Lausanne, thus giving us an opportunity to enjoy the scenery through the southern part of the Swiss Alps to the Simplon tunnel, with all the castles, quaint villages, grape terraces, ravines, snow-covered peaks. Finally we came to the bound-

ary. Foolishly, I did not provide myself with through tickets and had no Italian currency. I had to change money rather hastily at Domodossola, where everybody tries to take advantage of the tourists, and had some unpleasant experiences with fraud. The third-class crowd was becoming noisier at each station. We changed at Milan to a second-class compartment and proceeded to Genoa in relative peace. From there, we went directly to Rome.

May 10. My first visit was to the International Institute of Agriculture, situated in the beautiful park of the Villa Borghese. Various scientists were beginning to gather for the forthcoming Soil Conference. The Italians have done a very poor job in arranging the meetings. There were mix-ups everywhere, from hotel reservations to scientific programs. But the city is beautiful and the people are charming. In the afternoon, we visited St. Peter's Cathedral. No words can express the magnitude of the Piazza and the vastness and beauty of the cathedral. Had a view of the city from the Villa Medici.

May 13. Spent most of next morning in the Russian Embassy, waiting for the consul to show up, so that I could apply for a Russian visa. When he finally arrived, I was received quite cordially. We chatted for an hour about America, Russia, and Italy. Both the consul and his secretary tried to convince me that Russia could make good use of American-trained men. The embassy itself still seems to be dominated by the old Russian system. To obtain an application blank, I have to come again in a day or two, since the key to the office where the papers are kept has been lost or was carried off by someone.

An official reception was arranged in the evening for members of the Soil Conference. The only Russian who has arrived is Glinka, a fine scientist and a charming personality. There is also the German Ramann, a towering individual, who is half-deaf. The Czech Stoklasa made a rather bad impression, a superficial poseur rather than a scientist. There were many others, including numerous politicians rather than scientists.

The conference itself was opened officially by the King of Italy. There was quite a flourish of speeches from the many delegates. Most of them were too long. Glinka was the only one who tried to infuse a little life, by pointing out that science knows no national barriers. A

committee was appointed to organize a permanent Association of Soil Science. The afternoon was devoted to scientific discussions.

The next day, the conference broke up into several sections. A most haphazard combination of subjects and languages, without rhyme or reason. Stoklasa brought with him a group of students, all of whom talked a lot, but contributed little. No new word has been said so far. Largely because of a shortage of microbiologists and probably because the next congress is to be held in America, I was asked to serve as one of the vice-chairmen of the Commission on Soil Microbiology. That afternoon, I presented two papers, one on methods in soil microbiology and the other on sulfur oxidation. This led to a rather curious coincidence. The Italian G. Rossi also presented a paper on methods. He became rather nervous when he saw my paper on the program. He called me out into the corridor and suggested that we should come to some understanding on procedures. I pacified him as well as I could by pointing out that scientific work can afford to be duplicated. He insisted, however, that a resolution be adopted and that a special committee be appointed for standardizing methods. The whole thing appeared rather ridiculous. How can one standardize methods in soil microbiology when we do not understand even the significance of such methods? I was glad that Winogradsky finally arrived at the conference, so that he could help me out in such an emergency.

May 14. Met a lot of very curious people, who, under the guise of soil bacteriology or "microbiology," a term that has not as yet come into popular use in Europe, manage to pull the wool over people's eyes and play politics. Some are bombastic. They seem to know something of the earlier history of the subject, but not the latest contributions. They appear to be little interested in the subject. There are only a very small group, perhaps five or six, who are seriously interested in advancing our knowledge of soil microorganisms.

Prof. Wiegner of Switzerland, one of the clearest minds attending the conference, gave a brilliant exposition of soil colloids. Winogradsky made a masterly presentation of the lack of appreciation of soil bacteriology proper and proposed a new method of approach. There were other addresses which were rather poor and incoherent.

In the evening, we went to a reception at the Capitolium. Beauti-

ful halls and rooms, fine old statuary, illuminated garden, large and merry crowds, charming ladies, music, refreshments. After supper, went to see Glinka, who promised to help us obtain a Russian visa.

May 20. Went for a trip to Mt. Albans in company of a large number of pedologists.

Had a glimpse of the false internationalism of some of our conferences. Had a special argument with Prof. Aarnio from Helsingfors, who said: "Why, the Russian people have contributed nothing to world civilization, they have no right to exist, they are barbarians and will always remain so; especially since they allow themselves to be governed now by a group of Jews. The Russian soldiers who made the revolution in Finland were all killed, every one of them; those Finns who helped the revolutionists had to pay for it dearly." Some agreed with all that he said. To change the subject, I asked him whether they were planning to come to the next Soil Congress, which is scheduled three years hence in America. He answered, laughing, that of course he would, but "America must send a ship for us; America has plenty of money; it possesses now all that Europe has lost and can thus well afford it; there are no soil scientists in America anyway, the few that are there do not amount to anything; they may thus have an opportunity to learn something." Another gentleman, speaking of the relations between the various countries, said: "We must be nationals first of all, and only then scientists." I was amazed and disgusted. And these men called themselves scientists!

The last day of the congress, Stoklasa insisted that he be the president of the Commission on Soil Microbiology and have one of his assistants as secretary. All my objections were of no avail. Dr. Lipman was elected president of the society. Four vice-chairmen were then added to the third commission, among them myself.

We left by train for Naples with a group of congress participants. It became hot. At the stations, we saw groups of ragged Fascists, who were constantly making noise and exhibiting guns. Visited the sulfateras; were amazed at the tufa rock, which is made up of volcanic dust. The scenery is matchless.

Saw the bacteriologist G. Rossi, who has a fine and well-equipped laboratory. His major problem is the study of retting of textiles. His

laboratory is located in a chapel of the old Bourbon dynasty.

According to an old Italian engineer whom we met at the congress: "The Neapolitan is poor, he likes to think, to sing, and to enjoy life; he has had a fine history, no gladiators, no inquisitions; he is liberty-loving, hard-working. He has given up his liberty for the unification of Italy. Now, Mussolini is causing great dissatisfaction among the people. The Neapolitan is prolific, but morally pure. If the soil were only handled properly, if other agricultural resources were only properly utilized, there would be no emigration question. The government tries to support the metal industries, which are foreign to Italy, and not the native silk culture and linen industry."

May 26. On our way from Naples northward, we stopped in Rome, to visit the Russian consul, who said that although no Russian visa had as yet arrived, we would have no difficulty; the visa would await us in Berlin.

Made a day stopover in Perugia, where we visited G. de Rossi, at the Agricultural Institute; his laboratory is situated in one of the cells of an old Benedictine monastery. Equipment is not of the best, except for a few new pieces of apparatus, obtained from Germany through the Reparation Commission. Rossi has a fine sense of humor.

May 30. On our way to Florence, we enjoyed very beautiful scenery: the hills of Umbria and Tuscany, the towns perched on the tops of the hills, the narrow valleys, the primitive agriculture. A Fascist guard was collecting our tickets and discovered that we were using a wrong route. He cursed the conductor for permitting us to do that. Landed in a dirty, bedbug-infested hotel; had to get up in the middle of the night to search for a better place.

June 10. The trip to Milan gave us an opportunity to observe the northern Italians, who are far more energetic than the southern Italians; a totally different race. Visited the cathedral, the cemetery, the Agricultural Institute. Saw Prof. Carbone, big, large-bellied, near-sighted, good-natured bacteriologist. He took me to see the Serotherapeutic Institute. He also works on retting and claims that *his* cultures are the true retting organisms. The Institute was established thirty years ago by the city for the manufacture of diphtheria antitoxin. It has now developed into a self-supporting organization, employing two

hundred persons and manufacturing various microbiological products, including some for the treatment of infections and others for industrial purposes. Spent an hour with Prof. Gorini, a fine old gentleman, interested in cheese preparation; has a very small laboratory, but many cultures; the incubator and sterilizing chambers look like a cellar full of rubbish.

We left for Lausanne in company of three young Irish priests who were coming from Rome. Discussed the Catholic conception of the universe; they proved to be very intelligent and fine young people, interested in Gaelic culture.

Arrived in Yens and found Byron in fine condition. Took him to Geneva to buy some clothes. Visited Prof. Chodat, the famous botanist, whose son we met in Rome. A splendid laboratory, large herbarium, fine botanical library, and especially an excellent collection of pure cultures of algae, certain molds, yeasts, and bacteria. For a city of its size, Geneva has had the greatest intellectual development. Théodore de Saussure, who first demonstrated the use of carbon dioxide by plants, is considered the greatest scientist who ever lived in Geneva. As a result of the high rate of exchange of Swiss money and the Russian breakdown, Geneva is suffering. The university, which, before the revolution, had seventy per cent Russian students, is affected particularly. Visited Prof. Lindner, at the Pharmacy School, with whom I discussed the importance of fungi as agents of cellulose decomposition. On our return to Yens, it began to rain. The locomotive had a hard time pulling uphill, and finally broke down. It was fixed with a few splinters, and we finally arrived at our destination.

June 13. We said good-by to Byron and left for Bern. Visited the suburban town of Liebefeld, with its Experimental Station, where I met Prof. Burri, a brilliant bacteriologist with a fine sense of humor. He is working on the staining of bacteria, the isolation of single cells, and their cultivation in deep agar tubes. Other problems observed dealt with the occurrence of lactic acid bacteria in soils following prolonged treatment with manure from silo-fed cattle, and the effect of carbohydrates, especially sulfite residue, upon nitrogen fixation.

Had a magnificent trip on Lake Thun, with the mountain ranges towering all around, to Interlaken. Went up a funicular, took some

fine walks, and had a day trip to Lauterbrunnen-Grindelwald. The upper glacier proved to be especially interesting.

From Interlaken we went to Zurich. Everywhere beautiful scenery, snow-capped mountains, lakes, waterfalls. The trip proved to be a little fatiguing. After examining several hotels, we finally stopped in St. Augustine Hof, a semireligious boarding house, very comfortable and rather inexpensive.

Here, I visited the Agricultural College, where I met Prof. Düg-geli, who teaches bacteriology and has some interest in soil bacteriology; Prof. Schroeter, the great old botanist, who did some fine work on peat; Prof. Wiegner, whom we met in Rome and who works on animal and plant nutrition and on soil colloids. Düg-geli believes that at present we know very little about soil microbiology; we are only at the beginning of the development of the science. Wiegner has a very clear conception of the complexity of the soil constituents. He believes that the simplest problems contribute the most. Gifted people go into physics, chemistry, and botany, and the ungifted ones go into agricultural sciences. There is a great lack of properly trained men in this field, where the greatest training is required.

Dr. Wiegner proved to be a charming host. He took us to the park along the lake. Here we drank beer, listened to music, and discussed the world and its problems. The agricultural chemist is a transitional species; he attacks soil problems in a superficial manner, working with a complex medium and attempting to make too-broad generalizations. The chemist and the physicist are shy of the complex soil problems. These are, therefore, attacked by men with insufficient scientific preparation. Instead of true scientists, men trained in agriculture are utilized; their viewpoint is narrow, their training insufficient. Dr. Wiegner and his wife are highly sympathetic people; they are Germans and feel rather lonesome among the self-satisfied Swiss.

June 24. From Zurich we traveled to Munich, amidst rain and clouds, in a rather dreary atmosphere and among dreary people. The city is beautiful. The people appear to be recovering from some serious illness: some are happy, some are sad, with strange, hungry looks on their faces; others are resentful, and some unfriendly. Everybody drinks beer in quantity. We visited museums, palaces, churches, in-

stitutes. The city is full of theaters, beautiful parks, cafés with orchestras, all of which go to make the city a place of beauty, where one would like to live and enjoy oneself. There are plenty of well-dressed people; large crowds eagerly examine the wares in the windows. In the evening we went to a Russian cabaret, where we had a good laugh at some Russian jokes and entertainment.

Visited Prof. Ramann in the Forestry Institute, where we also met Dr. Kraus, a good soil chemist. The old concept of humus as a single chemical substance or mixture of substances still prevails here. Scientific research has disintegrated in Germany; no improvement is foreseen for the near future. The outlook is rather pessimistic; only the practical things are encouraged. Long-term experiments, requiring great patience, have to be given up. The forest pathologist is even more pessimistic. The soil bacteriologist Hiltner, has just died; there is no one to take his place, and his laboratory is disorganized. America is much resented for having concentrated all the money.

The famous Peat Institute has become a technological organization, in which the technician is taking the upper hand; the chemist has to limit himself entirely to routine analyses. Any research work ever done is carried out of one's own free will and remains unappreciated. "There is already enough theory; we must apply it now to practice." The same is true of the Hiltner Institute: a magnificent building, limited largely to extension work, to supplying spraying materials, bacterial cultures, and information to farmers. There is a third-rate bacteriologist who has only a very faint idea of what soil microbiology is. The only thing he is working on now is the development of non-nitrogen-fixing bacteria which would stimulate the growth of beets and other plants. His idea is that these bacteria supply energy to the nitrogen-fixing forms, thus indirectly influencing the growth of plants. Crude cultures are used. I could not see the cultures nor was I told what media were used.

I was much impressed by a visit to Prof. Richard Willstätter of the Chemistry Department at the university and his ideas on experimental methods in biology, on the place of the scientist in society. Little did I dream that only a few days after our meeting, Willstätter would resign from the university as protest against its anti-Semitic policies, the forerunners of Naziism.

June 27. From Munich we went to Dresden. The day was bright and the scenery magnificent, first the fields and forests of Bavaria, then the towns and factories of Saxony.

Visited the Agricultural Institute, where we saw soil nematodes feeding on soil algae, where soil inoculants (Azotogen) were being manufactured. Met some old friends, a chemist and his wife, who used to live in New Jersey. They are longing to go back to America. People live here very poorly, buying no new clothing, eating the cheapest foods. In the evening, in what appeared to be a rather nice place, we feasted on chicken and were sick later. "Who eats chicken in Germany during the summer? Only stupid Americans."

Spent much time visiting galleries, with their fine collections of paintings, the castle, and attended an opera in the evening, where we saw a fine Wagnerian performance. We were struck particularly by the entr'actes, during which everybody unwrapped packages of sandwiches and proceeded to eat and drink. There was little merriment in the crowd; no luxurious or rich dresses. Just plain people, quietly chewing their sandwiches and drinking their beer. They drink here much less than in Munich. A German whom we met said: "Nationalism goes well with a great deal of beer-drinking and big bellies." A great many children are begging on the streets, complaining "*Nichts zu essen.*" We have not seen this since we left Naples.

July 1. We spent several days in Leipzig visiting its magnificent institutions. At the Agricultural Institute, I found a good bacteriologist, Dr. Zipfel, interested in pursuing scientific work, which does not suit very well the tastes of the practical men who are now running the institute. He is a naturalist, working on the classification of legume bacteria by serological methods. He complained that the German soil investigators are not properly trained; they are practical men, not scientists. Everybody is making pH determinations, because it is in style now, and everybody wants to be in the limelight. I met the botanist, or rather plant protectionist, Zade, a student of Hiltner. When I suggested that the latter's latest scientific contributions were not very significant, he mistook my remark as derogatory to his teacher and exclaimed: "Hiltner's contributions to science are greater than those made by American scientists, all taken together." I tried to pacify him, but our conversation ended abruptly.

At the Botanical Institute, I found not only a better reception, but a finer scientific spirit. Prof. Ruhland has replaced the famous botanist Pfeffer, who died only five or six years ago. There used to be many students, but they suddenly stopped coming, especially the Americans. Czapek took Pfeffer's place, but he had to serve in the Army five years as physician; his health broke down, and he died about a year or so ago. Pfeffer's library was sold to Japan; Czapek's was given to Louvain University. Now the Botanical Institute has to start all over again, organizing a new library. Ruhland himself is doing fine work on the bacteria oxidizing hydrogen.

Had a fine chat with Prof. Wolfgang Ostwald, the physical chemist. He worked with Jacques Loeb in California, together with Robertson. He has the best feelings for Loeb, but considers his protein theory as incorrect, his pH work as rather weak. He believes that in general the pH work has been greatly overestimated and misdirected. Loeb could only make such errors because he was not a chemist and especially since he did not bother to read the work of others. Loeb's book on tropisms was included among "*Die Klassiker der Wissenschaft*," since it is an excellent work, but this is not true of his book on proteins. Ostwald himself is a short, stubby fellow, with a big belly. He is rather humorous, has many students, whose names he does not even bother to remember.

Spent some time in bookshops and in line waiting for some money in the bank. Waiting has become a habit in Germany, especially in Saxony; everywhere you see waiting lines, before the railway ticket windows, in milk stores; but the people do not seem to mind.

July 3. Proceeded to Halle, a small, but very busy, town. The streets are crooked and the institutes are widely distributed. Prof. Lang, whom we met in Rome, and his wife came to visit us at our hotel yesterday evening, and we spent a couple of hours drinking beer. Lang is a geologist, concerned with rock weathering and soil formation. He has no laboratory and spends only a few hours a week in the institute. Visited the biochemist Abderhalden, a good-natured, middle-aged, very busy gentleman, not half so bad as his long name and title of Geheimrat might indicate. This may be because he is a Swiss working in a German university. We spent much time discussing enzymes

and microorganisms. He extended to me an invitation to write a section for his *Handbuch*. He complained of lack of frankness in the German scientist, his love for details. He admires the French genius, the friendliness and openheartedness of the Russian and of the Englishman, and considers the American as rather sensitive, but full of potentialities. He has seen so much suffering that he cannot understand how people can talk of another war.

Had a long talk with Dr. Schneidewind, who formerly made some important contributions to soil microbiology. Now, he is interested only in fertilizer experiments. He has little use for science in general and for soil microbiology in particular; he spoke of this field of science as "nitrogen fixation and that sort of thing." I could understand his attitude when I visited the laboratory of the bacteriologist working there. It was about on a par with that of Perotti in Rome. It was a miserable place: a small room, full of dust, with a large table covered with dried-up soybean plants and a number of flasks with some dried-up bacterial growths on the bottom. The cultures appeared to be at least a year old, as one could no doubt measure more accurately by the thickness of the dust layer on the rubber plugs. After talking to these people, I left, discouraged and disgusted. I begin to feel somewhat tired of hunting up these so-called soil microbiologists. They make a lot of noise and certainly do much harm to the science.

July 6. We are now in Berlin. Started early in the morning for the Russian consulate to ask for the visa for which I applied and which was promised to me in Rome. I was told to come later.

Have been visiting the agricultural schools in Berlin and in Dahlem. A lot of work, of questionable value, is carried out on symbiotic bacteria, not of the leguminous type. Miede has done some fine work on the self-heating of hay; this was considered as a biological process to begin with, later followed by a series of chemical reactions. When the hay is heated, it takes fire.

The plant pathologist Prof. Appel and his colleagues, notably Dr. Behn, were very friendly. The latter gave me a fairly clear picture of soil microbiology in Germany and emphasized the harmful effect of Löhnis upon the younger workers. He drew a very sad picture indeed. Prof. Lemmermann, the agricultural chemist, rules his department

with a strong hand. Nobody is allowed even to go to the library during the regular hours without his permission. He himself seems aloof and superior. He talks readily, however. He believes that lack of sufficient interest in soil bacteriology in Germany is only a passing phenomenon. According to him, this science has only limited practical possibilities and, as such, will take its place among the other agricultural sciences. He emphasized also that some of the earlier soil bacteriologists in Germany, notably Hiltner and Löhnis, are mostly responsible for this low state of soil microbiology, since they put forth many promises which were never fulfilled.

Visited the botanist Warburg, who has just returned from Palestine, where he is spending most of his time. He spoke of extensive anti-Semitism in the German universities, due to the underlying economic difficulties which are a result of the postwar period. He spoke of the progress made in Palestine, expressing great hopes for its future. Saw Prof. Schucht, who treated me to some whisky in a porcelain tumbler. We discussed soil science problems. According to his concept, one must be a geologist who works on soils, or an agricultural chemist who analyzes soils, or a botanist who studies soil bacteria, but not a microbiologist. "Who wants soil bacteriology, anyway?"

Also spent a morning with Prof. Hans Pringsheim, who was an associate of Emil Fischer's and now has charge of one of the chemical laboratories. He formerly worked in soil bacteriology and considered himself at that time as one of the more prominent younger workers in the field. When he came to Fischer, he took up the chemistry of the polysaccharides. He believes that the field of soil bacteriology is very limited and does not lead to many practical results. He considers the work done in America on cellulose bacteria as of inferior value. Left him also in a depressed mood.

Saw a well-equipped plant for alcohol brewing and acetic acid manufacture. Spent an hour with Dr. Haehen, a pupil of Buchner's, who is working on the nature of the hydrolytic and oxidative enzymes. He is starting a new journal on the chemistry of cells and tissues. He showed me an article by Stoklasa, in which inoculation of soil with phosphate bacteria was declared as one of the greatest contributions to modern science. I told Haehen that this was a lot of nonsense.

July 14. Visited Pringsheim at his home, where I met several of

his former students, a rather uninteresting lot. Pringsheim was very careful in talking about politics and considers Haehen's work as worthless; he expressed the same opinion of the work of certain other bacteriologists and chemists. After looking at some of the two hundred papers that Pringsheim himself has written, I would be inclined to say that some of his own work, as well, is hardly of great significance.

In Dahlem I met some very brilliant biochemists, working at one time or another on microbiological problems. Otto Warburg demonstrated to me his studies on the respiration of algae and of cancer tissues. His apparatus is rather ingenious. Meyerhof has just accepted a position in Warburg's laboratory, and I met him just as I was leaving. Visited Neuberg, who, after making me wait for about an hour, received me, informally, lying on a couch. He proved to be very congenial, however; he talked a lot, trying to make me feel at home. He mentioned to me that the Russian bacteriologist Omeliansky is now in Berlin, on his way to Paris. I started out at once to search for him. I found him in a small, dingy little room in a boarding house. We spent the whole afternoon talking about Russia, the revolution, bacteriology, and the world at large. He is a professor at Leningrad. For a two-months' trip to Western Europe he was allowed the pitiful sum of two hundred rubles; he is now waiting for some more money. The older men are dying off in Russia and there are no young men to take their places; training in biochemistry is entirely lacking.

In company of Omeliansky, I went to the Russian Commerce Division, asking them to intervene in my behalf and help me obtain a Russian visa. My telegrams to Moscow and Omeliansky's help have had their effect.

July 25. Late in the afternoon, we departed by train for Warsaw. The winter grain in the fields has just been cut; the hay is poor. The late summer grain also looks poor. We see again those villages, churches, and farmhouses that we were so accustomed to in childhood. Groups of small straw-thatched houses, surrounded by a few trees, usually birches. The potatoes look good, but the beets seem poor. The larger estates show much better stands of grain. There is a bare-foot boy, then a peasant emerging from a haystack. An occasional, poor-looking cow.

After a sleepless night in the train, we spent a day on the streets

of Warsaw. On passing through Lodz, we had the first glimpse of the Polish Jews, whom we have never seen before in their native environment. They looked rather pitiful, especially the young men, with the faint beards on their faces, their long coats and their small caps. There was quite a contrast between them and the Slavic Poles. One does not wonder that great antagonism has existed for so long between these two peoples.

August 14. The overnight trip from Warsaw to the Russian boundary was uneventful. We had the whole international car to ourselves. In the morning, we arrived at a dreary Polish station. Traces of Russian life and of Russian influence were seen everywhere, in the houses, in the faces of the people. The porters were very polite to us, Americans. The chauvinistic, self-satisfied Polish officials, in their shiny military uniforms, gave us the impression that they had just stepped out of the pages of Polish history of two centuries ago. Our baggage was removed for inspection; we were the only foreigners crossing into Russia. The official who made the examination, suspecting that we were returning Russians, began to mess up our bags. When I addressed him in English, however, he became very polite and closed our bags in a hurry. Not many visitors enter Russia by way of the Ukraine.

The train crossing the boundary consisted of one locomotive, one passenger car, and a few freight cars. The distance of fifty miles to the Russian station Shepetovka took nearly three hours. We were accompanied by a Polish gendarme and by a Soviet official. The latter could not be distinguished from an ordinary workingman. He kept apart from the Poles but entered readily into a conversation with us, giving us a few glimpses of life in the new Russia. The train soon approached the neutral zone, where the Polish official dismounted. The approach to the Russian line was very slow, as if the train were afraid of something. There was no difference in the topography of the land, in the crops, in the appearance of the houses, churches, or the people. The train stopped finally at the boundary. There was a Russian policeman mounted on a pitiful-looking white horse. The Polish gendarme looked down his nose. The appearance of this policeman, in the beating rain, gave an impression of simplicity and humbleness, in contrast

to the haughty appearance of those Russian policemen who guarded the boundaries in former times.

We arrived in the first Russian town of Shepetovka about noon. We had to wait in the station two hours for our train, which was to take us on the way to Priluka. The crowd at the station was indescribable; the great variety of very shabby clothes emphasized the prevailing poverty and misery. The crowded station reminded us of times past, the peasants in their typical garb, the small number of well-dressed people. But there was something new. First of all the newspapers, the colored posters, the free conversations of the people—all of whom spoke of a new world. Never before in all our journey had we found such striking differences between peoples of two different countries as we observed here, just within a few hours, by merely crossing an artificial boundary line. The childish curiosity of the Russian people, when they heard that we were from America, a certain new manner and speech, which was quite distinct from what we had known before, a total lack of the old type of soldier and the presence of a new type of youngish man, who, although somewhat slovenly dressed, was polite and respectful. There was a lack of small change but an abundance of dirty paper money. The railway fare was very cheap. Our baggage went through another thorough examination. Two young fellows, one a mere boy, became very curious about the printed material. Finally, they became interested in one of the illustrated magazines that we carried with us. I presented them with one. They were so delighted that our baggage was at once closed.

Finally the train pulled out. We were a constant source of wonder to all the passengers, who surrounded us and questioned us about America. We were approaching old familiar places. There were traces of destruction everywhere: broken buildings, half-destroyed cars standing on side lines, crooked, torn-up tracks, lack of renovation, broken-down fences, ruined cemeteries, gloomy towns. As we passed the city Berdichev, which used to be the center of our business and intellectual life, the gloomy impression was heightened. Everything appeared far worse than what we had left fourteen years previously. The only saving feature was the better relations and friendlier attitude between people, the disappearance of the classes.

The train carried only hard-seated cars. We arrived late in the evening at the station of Kaziatin, where we had to change at once to a train for Vinnitsa. When we entered the new train, the magic word again spread, "Americans are on this train." Everybody pricked up his ears. It was near midnight when we arrived in Vinnitsa. When we made inquiries about connections for Turbov, the nearest station to Priluka on the narrow-gage line, we were advised to spend the night in Vinnitsa.

We took a horse-drawn carriage, of the good old vintage, to a hotel in the center of the city. It was dark and drizzling. The iron shoes of the horses produced the characteristic sounds on the cobblestone streets. Here was the city where my father and grandfather were born and lived, here is where I spent many a day in my youth. Even the dim light showed traces of destruction everywhere. We finally arrived at what was once a very famous hotel. Partly broken stairs, poor lighting, doors that did not close; poverty everywhere. We were led into a room which was worse than anything that we had so far encountered on our journey. Two beds with suspicious-looking mattresses and no bed-clothes. On request, one pair of sheets was delivered to us; another request produced pillows; but quilts were lacking, and it was very chilly that midnight hour of our return to our homeland. After serious urging of the night clerk, two blankets were brought to us; but a second pair of sheets could not be found in the whole hotel. There was no food at all, not even hot water to make tea. We were hungry and shivering from cold, but we were so tired from the journey that even the hard mattress and the probability of insect visitors could not keep us from falling fast asleep.

The first impression produced by this once gay and beautiful city was indescribable, as if the whole world were ruined, in a state of destruction. The people looked shabby, though not hungry. One could hardly think of this as the emergence of a new world.

I walked through the streets as if in a dream, barely recognizing the houses that I knew so well only fourteen years before. This was a different world, a dirty world, a shabby and ragged world. Many of the houses were half destroyed, the pavement was broken up. I went to see the house where my father was born and which my grandfather had built and inhabited. There used to be a department store on the

corner of the street. It was gone now. The windows of the building were broken and the place was converted into a cheap restaurant. My father's house, a few doors away, was quite unrecognizable. The people living in the house came out to look me over. Some of them recognized me and rushed to hug and kiss me. Before long they began to pour out their woes and misfortunes to me.

I returned to the hotel, where Bobili was waiting to have some breakfast. None could be obtained, since there was no restaurant either in the hotel or in the immediate neighborhood. We could not even get tea. Fortunately, rumors of our arrival had spread. Various relatives began to appear in our room. They began to argue as to who should take care of us. We were led to a side street, meeting on the way more and more people, and finally arrived at the home of one of my former teachers, who had married a cousin of Bobili's. The rest of the day we kept meeting residents of Priluka who happened to be living in or visiting Vinnitsa. The whole day was spent in an uproar. Everybody complained, everyone had to tell his own story of misery. Dissatisfaction with the present regime prevailed. Many of the younger people were, however, in favor of it, referring to the others as "nep-men." We visited the theater where people gathered to celebrate the tenth anniversary of the beginning of the World War. We were led behind the stage to meet some of the performers. The children appeared to be the only ones taken care of, with many special homes being erected for their needs. The songs of the crowds were lively and stirring. My first impression was that this was a new world, a world that went back eighteen centuries to the origin of Christianity. The same ardent beliefs, the same fanaticism, the same need of idols, of new holidays. A new religion was being created. It was difficult, however, to hide the fact that many of the people were unhappy, especially the middle-aged and older people, not only the former merchants, but also the workingmen. Waste and red tape were evident everywhere. The police of the Czar were replaced by the rulers of the party. All non-Communists, even those occupying responsible positions, would sooner or later be replaced, when Communists could be found or trained.

Our friends lived very poorly, but they had adjusted themselves to the new conditions, occupying positions in some of the numerous of-

fices and having an opportunity to educate their children. They emphasized constantly that the situation in Priluka was even worse. We finally met two of our childhood friends, Meier Steinberg and his wife Masha. She was the flame of most of our youthful hearts, notably that of Peisi. She had been waiting for him on his return in 1917. When they met again, however, they were disappointed in each other. She married the only remaining member of our youthful group, who lacked the courage to leave Priluka. They both made, upon us, the terrible impression of people who had finished living and were now dragging out an existence. They had no spark of hope left; only complaint.

The following morning, we departed for the railroad station, on our way to Priluka, accompanied by a large crowd of friends and relatives (the train made the trip only once in two days). We had to wait two hours before the narrow-gage train for Turbov finally got started. There were beggars of all descriptions at every turn; there were waiting lines everywhere; human patience can endure a great deal. It took nearly three hours to make the twenty-mile journey to Turbov. We were scrutinized by everyone in the train and at the various railroad stops, as if we had just dropped down from Mars.

When the peasants and others on the train heard who we were, they surrounded us and began to pour out their feelings toward the present regime. Not one was favorable. A young fellow, a graduate of an agricultural school and the son of a once-wealthy peasant, said that the regime is bound to fall; the peasants are poor to an extreme; this year they will not get back enough grain to equal the planted seed; the land is not cared for, the seeds are poor, and the peasants are very much dissatisfied. A teacher from a neighboring village complained that there is nothing to do now in Russia; one does not read any book or newspaper; only communistic literature, for propaganda purposes, is available; he was reading with great relish a little article on popular science published some ten years ago.

From the windows of the slow-moving train we saw impoverished fields, with more weeds than oats and buckwheat. Finally, we arrived at the Turbov station, the point of our former departures for the outside world. With mixed feelings of joy and despair, we saw a large crowd waiting for us. Bobili's parents, brothers, and other relatives

and friends were there to meet us. We were placed in a wagon, driven by the same coachman who used to drive me in my youthful days, when I would depart from Priluka for the great world beyond. Along broken roads, we proceeded to our home town.

Finally we perceived the straw-thatched houses of Priluka! What a picture! Many were in ruins, most were shabby, and only few in fair condition. The people were in rags. There were large numbers of dirty children and elderly people; very few young and middle-aged men. Our entrance was like a triumphal procession. Everything alive poured out to meet us. I felt like falling to the ground and weeping, not sentimental tears, but the bitter tears of a son coming back to his motherland and finding her destroyed, her children reduced to the lowest state of degradation, their means of living gone. How can they be helped, and who is to help them?

I can no better express my feelings than by citing the entry in my diary, dated July 30, 1924, and written in Priluka itself:

"The greatest misery that one could ever imagine, the greatest catastrophe that the peoples in Russia have gone through; the greatest experiment in social and political relations of men can hardly express what we have seen during the last few days. Human life lost all vestige of value. A human being dressed in rags, living in a sometimes half-broken-down house, feeding on meat once a week. I have seen here, in the town where I was born and brought up, among the people whom I loved, human misery as it could only be imagined. The first impression was as if I were walking through a cemetery, with a few living ghosts wandering among the ruins. This misery overshadows by far in my thoughts the greatest experiment in human relations ever attempted, namely the communistic regime in Russia. It is rather difficult to decide with what to start; the head is full of impressions. Here I am sitting on a bench, a simple board on two projections, in front of the house, and looking upon the big open space, the center of Priluka. Near me is sitting Bobili Baron, my own Bobili's cousin. She, who formerly glorified in her education and social position, is now a poor creature, dressed in rags, sitting or standing, quiet, not saying a word but merely looking on."

We spent ten days in Priluka. We did not have one minute of rest. People kept coming in and out of the house all the time. Each

one had a tale of woe. Each one suffered to an extreme. Many cried like children before their father; they came to pour out before us all their sufferings. Some could not even talk, but stood in the corner and cried quietly. They looked at me. But who can describe it all! The mere sight of Mendel Rabin, one of my old teachers, of the town fool Bera, of old and young, brought shivers to my body. What had become of my town? How shall I ever forget you! I could not look the people in the face. I felt as if I myself were partly guilty for their suffering. Like dumb animals, they came to me for help, and none was forthcoming. The peasants of the town as well, the older ones particularly, came with similar stories. Only the young ones were more or less enthusiastic. I do not know how much this enthusiasm could hide the remnants of the Petlura * spirit in some of them.

Most of the stores were closed. My own home, built by my mother and in which I had been born, looked like a hole of a troglodyte. The room in which I had spent my childhood and youth seemed sad indeed. People emerged everywhere from their holelike homes to tell us of their woe. They all looked as if they were ashamed of something. The few remaining stores presented a miserable appearance. Only the green fields surrounding the town were the same; they showed the least traces of destruction. The young men came to me asking for advice as to what they should do. America was their only hope. Only there could they look for help. They found themselves between the hammer and the anvil. High taxes prevented the merchants from continuing their business. The laborers, carpenters, tailors, blacksmiths, and others found little to do, chiefly because of the poverty of the people.

Ukraine presented a sad picture indeed. The war, the revolution, the pogroms, the numerous roving bands, the difficult adjustment to a new form of government—all worked toward the complete effacement of the small towns. The only hope for the middle classes was in taking up land. Various collectives were being organized among the townspeople to go to the land, and the movement spread very rapidly. I fear, however, that a lack of proper training and a lack of funds will work havoc with them. Only absolute starvation in the home town can

* A Ukrainian bandit, called by some a Ukrainian patriot, who ravished this region after the revolution and during the civil war, before the Bolsheviks established their power.

keep them in the agricultural collectives. The mere giving of help is a palliative which will not solve the problem. The peasants, although at present depressed, have hopes for the future. The temporary ills and the lack of order will pass; a new life will come, when conditions will become more settled and the peasant himself will become more educated. There is very little future, however, for the Jews in the Ukraine, under this or any other government. America and Palestine are out of the question. Salvation must come from Russia itself. No one dreamt yet of the industrial development, which alone could bring salvation.

The day of our departure came. We bade good-by again to Priluka and proceeded by wagon, then by train, back to Kaziatin. Here we had to spend a day and a night waiting for a train. The same picture was found here, but it too was in a somewhat different form. Since this was an important railway center, many people seemed to be able to manage things better. The railway workers appeared to be well fed and well dressed.

On our leisurely train trip to Moscow, we stopped at various stations sufficiently long to enable us to gather impressions. Many of the cities on the way presented a picture of utter destruction. Armed soldiers accompanied the train; broken-down railway cars and ruined railroad structures were everywhere. The mud huts of the Ukrainian villages gradually gave way to the wooden houses of central Russia, just as the black soil gave way to the gray podsol and to a region of great forests. The second-class car in which we were riding was full of Soviet officials, going about their business of handling the trade of the country as well as the government.

After a day's journey we arrived in the capital city of Russia. The peasants, in their ragged outfits, pouring into the station, the numerous church steeples, the droshkis: all reminded us again of a new world. There was Peisi to meet us, happy, neatly dressed. He took us to his poorly furnished but large and well-lighted room on the fourth floor of a former hotel. There was his wife Mara. Both seemed to be happy in their life and work. Since Peisi's family was spending the summer in a suburb, or *dacha*, we had the use of his room in the city.

I went off at once in search of scientists. My first visit was to the soil division of the Ministry of Agriculture. The place presented a rather shabby appearance. Later, I went to the Bakteriologo-Agronomi-

cal Station, where I met two young investigators, both bright and intelligent. They gave me a good picture of what was being done in the field of soil bacteriology and in other branches of microbiology in their own institute and in others.

I also visited Prof. Bach at his new Institute of Chemistry, a venerable old man, bearded, and accompanied by a big brown dog. His laboratory, a former palace, had magnificent carpets on the floor. Bach had returned to Russia in 1917, after a thirty-year absence as a political refugee and had come to Omeliansky to ask for the use of a small laboratory, so that he could continue his scientific work. This was refused. When I met Omeliansky in Berlin, he commented about this: "We have enough revolutionists in Russia and do not need to import them from abroad." Bach then proceeded to see Lenin, an old friend who had lived in Bach's house in Geneva, Switzerland, while an exile. Bach is said to have addressed Lenin as follows: "What sort of a revolution have you made here! You know that I have been away for three decades, and upon my return I cannot find a place to work." Lenin was supposed to have replied: "These old conservatives, they will not allow a revolutionist to do scientific work. I will show them!" He then proceeded to fit out a whole institute for Bach.

We also visited various museums and galleries. The Trediakov Museum occupies a magnificent building of the old Russian architecture and possesses a fine collection of Russian paintings.

Bobili decided to stay in the country with Peisi's family. This gave me more time to visit scientific institutions. I visited the famous Agricultural Institute, now known as the Timiriaziev Academy. There I met the agronomist Prof. Prianishnikov. He and his wife are allowed to occupy only one room. It is a large room, with a big table at one side, a smaller table in one corner, a piano in another corner, and books all around the room. Prianishnikov himself seems old and broken; he has suffered a great deal. He has to occupy several positions in order to live. There is no time left for thought or productive writing. They have only the bare necessities of life. The right to appoint assistants and to arrange the program of study is taken over by the council of the students. The institute has about four thousand students, twice as many as it can properly care for.

We went to see the laboratories and greenhouses. There was Bobko,

a very intelligent soil chemist, and Dojarenko, the agronomist. The latter is a vigorous person, healthy in mind and body, who knows what he wants and is ready to fight for it. His standing with the government is not of the best. He does not believe that there is a great need for fertilizers; proper cultivation and crop rotation will do. A walk through the fields seemed to justify his claims. The peasant plows late, since he has to use his land for pasture as long as possible; the land is, therefore, not worked properly. The microbiological work at the institute is carried on in co-operation with Prof. Chudiakov, who was away at the time. I also met Prof. Williams, a fantastic individual, who was being supported by the government. He made a very poor impression upon me; he seems to be more a politician than a scientist. He treats soil science from a logistic rather than a factual point of view. I was invited to deliver a lecture before a large gathering of professors and students. We had refreshments, inspected laboratories, discussed various microbiological and soil problems. Later I met Prof. Uspenski, who has charge of soil bacteriology at the university and at the Fertilizer Institute. He did not impress me as one who would revolutionize our science.

Accompanied by Bobili's younger brother, Misha, an engineer living in Leningrad, we proceeded to that city. On Omeliansky's recommendation, we were given magnificent rooms in the House of Scientists, which occupied a palace of one of the Czar's uncles. Our rooms overlooked the river Neva. The city itself was depressing. Grass was growing through the cobbles in many of the streets that are lined by magnificent palaces. Many of the palaces have been destroyed. Many more show broken windows. You see a large brick building gaping with numerous rectangular holes telling a sad story. Many of the streets have been renamed. "I was born here and spent all my life here," one scientist said to me, "but very often I find myself not knowing the name of the street where I am walking." The old names usually still cling, however. Even many of the old decorations of the Czar are still seen in the palaces. The destruction is great. A great many people are begging in the streets. Some of the people are well dressed and speak foreign languages, suggesting remnants of the old aristocracy. There are very few restaurants, the better ones charging very high prices.

We spent a day in Dietskoie Sielo (formerly Czarskoie Sielo, the

home of the czars), accompanied by Prof. Glinka, who is Dean of the Leningrad Agricultural Institute. We visited the palaces of Catherine the Great and of Nicolas II, where we were guided by a former servant of the latter. Many of the old monuments were still there, notably those of Peter and of Catherine. Some of them have new titles. The heavy monument of Alexander III sitting on a huge horse, in front of the railway station, has engraved on its pedestal: "Your father and your son were killed by the people for their evil deeds; you shall stand here as a scarecrow to tell future generations not to desire another one like you."

I met many scientists, some of whom are convinced that a monarchist restoration is bound to come, that a Russian army will march in from abroad, with American support. Have visited the physiologist London, who is a poet, a lover of music, a mathematician, and a very intelligent man. Had a different kind of reception at the home of the soil chemist Gedroiz. He is a very modest man and wore a black blouse and boots. His brother and nephew were shot, one daughter died in the typhus epidemic, the other daughter is now sick at home. His house and family were very depressing. One wonders how he can do such fine work under these circumstances.

I was given a reception by the microbiological section of the university. A dinner was prepared for us at the laboratory; everybody was anxious to show his friendliness. We visited the Ermitazsh Palace, the Museum of the Revolution, and others. Lenin has been raised to the status of a saint, which a new religion requires.

The last three days of our sojourn in Leningrad were spent in an effort to obtain an exit visa and permits to take out my own camera and various books that I had purchased. Standing in line at the custom office for final signature, I found myself behind a tall, well-dressed gentleman. He proved to be a famous zoologist, who had been invited by the Royal Society of England to deliver a lecture in London on typhus fever. He held in his hand a small vial containing some fleas that he wanted to take with him for demonstration purposes. He was not allowed to take out the fleas without a proper permit. "There is no department in the Soviet Union which has a right to give permits for the export of fleas." The custom official finally advised the professor to get a letter from the president of the Medical Society of Leningrad recom-

mending that a permit be granted for such export. "I happen to be the president of the Medical Society!" "Well, then, write yourself such a letter." Apparently, the age of Gogol is not yet passed in Russia.

Finally we left Leningrad for the Finnish border. Misha accompanied us to the boundary. Baggage examination by the Soviet authorities was casual. Within a few minutes we were in Finland. What a contrast! We have again entered a new world! A neat little station. Only a small bridge separates it from the world we have just left. Incidentally, half of this bridge is painted red and the other half white.

September 3. Only one day was spent in Helsingfors, which allowed us enough time for visits to the university and to several institutes. Dr. Brenner, a capable bacteriologist, took us through the laboratories and the city. It is all so neat, in such marked contrast to what we saw in Russia. The cheapness and excellent quality of the food, the beautiful harbor and park, all left a very fine impression upon us. There is a tremendous dislike of everything Russian; the nationalistic Finns even rebel against the Swedish minority. Scientific impressions were not too profound. Possibly we missed seeing the prominent people, since it was vacation time.

We proceeded by train to Abo and then by boat to Stockholm. Various people whom we met on the trip gave us some fine points about life in Europe at present, such as the art of smuggling money out of Russia and the superiority of one Scandinavian people over another. We heard much of the lack of culture in America. On occasion, we were told, however, that it is the Scandinavians, possibly the English, the northern Germans, and a few Americans, that form the superior race of the world at present.

The first day in Stockholm was spent largely in sight-seeing, admiring the beauties of this northern city, situated on islands, with its magnificent parks, streets, museums, and scientific institutions. We visited Prof. v. Euler and discussed enzymes, Prof. Barthel and discussed soil microbiology, Prof. Hesselman and talked about forest soils, and Prof. v. Feilitzen, with whom we talked about soil fertility. We enjoyed the fine food and charm so characteristic of Swedish homes. We spent an evening at the home of Von Euler, where we met, in addition to some of our Swedish friends, several visiting scientists from Germany and from the Soviet Union. Von Euler comes from an old

aristocratic Bavarian family; he has many children, some by his first wife, and some by his second, a former student, now a co-worker. The dinner was a charming affair with many wines and a fine, friendly spirit. Von Euler takes interest also in art and literature, and was a painter of sorts at one time. One is struck by his hero worship of Napoleon.

Some of the Russian colleagues came to our hotel the next day to tell us a great many things about present-day Russia that cannot be discussed in Russia itself. They did not sympathize greatly with the Soviet rule but believed that it is the only system that can keep Russia together.

Our Swedish friends were most hospitable. The last two hours of our stay in Stockholm were spent in the home of Olaf Arrhenius, a two-century-old bungalow situated in a magnificent park. Although we had to make an early train that evening, we could not refuse Olaf's insistent invitation to dine with his family and friends. He saw to it that our baggage was on the train ahead of us.

September 6. Spent two days in Copenhagen and in Lyngby, where we were met by Prof. Harold Christensen, who took us through his new, well-equipped institute. He has some sound ideas about soil microbiology, but, unfortunately, he cannot bring them to a logical conclusion; he is too practical and does not see the value of pure scientific research. He has an excellent assistant, H. J. Jensen, who is working on soil actinomycetes, one of the few investigators I encountered in Europe studying this group of microbes. In the afternoon, I visited Prof. Orla-Jensen. He was critical of Bergey's manual, since it went much further in classifying bacteria on a physiological basis than he originally proposed. He disapproved greatly of Löhnis's ideas on the life cycles of bacteria, although he admitted a certain degree of bacterial variation. Also met Bondorff, who is teaching soil science at the Agricultural School and has some general interest in soil bacteriology.

We left by night train for Holland via Hamburg, arriving late at night in Deventer. We stopped at an attractive little hotel, where the food was not as good or as cheap as in Scandinavia. We were very tired and went to sleep, to be awakened early next morning by people coming to market, just under our windows. The wooden shoes on the cobblestone streets made a characteristic sound not to be forgotten.

We went, by bus, to visit Prof. W. Beijerinck. He is now retired and lives in the country, on the main road between Deventer and Zutphen. He occupies an old-fashioned house, with a small greenhouse, garden, and large park adjoining. A little old lady, sister of Beijerinck's, met us and made us quite comfortable. Soon Beijerinck himself came in and greeted me: "You are the actinomyces man!" He is very impressive, somewhat stooped, with a fine intelligent face, smooth shaven; his hands were shaking. He was dressed in a black suit with a large black tie. He talks good English, has to search for a word sometimes, and often mixes in Dutch phrases, which he soon translates himself. While Bobili went for a walk with Beijerinck's sister, we went to his study, warmed by a little stove. He complained of the fact that he had to leave his laboratory, although he feels that he could still go on with his work, especially now when he no longer has to teach students. He has to buy his own chemicals, and he carries on some experiments on nitrogen-fixing bacteria, using a few flasks and petri dishes. He prepared a special demonstration for me. He is keen at observing details; his lack of chemical training prevented him from carrying his work further than the ecological and botanical phases. Most of his studies have to be checked again and carefully worked out in detail by others. He has not followed the literature lately, because of a defect of the eyes, and is, therefore, not familiar with most of the latest contributions to soil microbiology. He is a kind person, does not hold himself proudly aloof, is friendly, and makes a profound impression both as a man and scientist.

We discussed various problems and personalities. He thinks highly of Winogradsky, although he is inclined to consider him a dilettante, who does not have to depend on his scientific work for a living. He was a little surprised when I told him we were coming from a visit to Russia, but he was glad to learn that we are not Communists. He invited us to have lunch with him, frugal but good. I wanted to take a snapshot of him, but he refused. Finally, at the suggestion of his sister, he agreed, somewhat nervously. He and his sister took us to the bus and expressed their hearty thanks for our visit. More about this great bacteriologist later.

We returned to Deventer and went directly to The Hague. We were so tired by the trip, and so fatigued by the numerous impressions,

that we did not care to see anything more. We proceeded immediately to Paris, sitting up the whole night, since the sleepers were expensive and our funds have been reduced to the vanishing point. Exhausted, we arrived early in the morning in Paris, where we engaged a room in a cheap pension on the Left Bank. We were much happier now than on our former visit, since life was simpler and the people in the pension more interesting.

September 13. Our little hotel proved to be a godsend. The weather is perfect. We are getting ready for our return trip. Our money will just about suffice, although we could make use of a little more, if we had it. Life in Paris is very cheap, once you get into the right swing. We spent most of the week shopping, going to the theater, where we saw magnificent plays, finely acted. We walked back late at night, happy and cheerful. I tried to complete the scientific excursions that I did not have time for on our earlier visit. The point was made time and time again that very few young Frenchmen go into laboratory work; they usually go into industry; research work requires long training and there is little compensation. Girls cannot be of much use here; they usually marry or look for more lucrative positions.

Again visited the Pasteur Institute, where we met Dr. Weinberg, a fine bacteriologist, a native of Odessa, who spent most of his life in Paris. He speaks a good deal of himself and his accomplishments, has gone from one field of medicine to another; he recognizes his lack of chemical training. He is intelligent and frank; he is not a leader in science, but an important contributor through his work on the anaerobic bacteria.

Sunday we spent again at the Winogradskys'. They were very glad to see us after our visit to Russia. We met there several Russians, including Berdniakov, now working at the Pasteur Institute. They were all anxious to hear about our Russian experiences and impressions. Winogradsky was definitely homesick. After a fine dinner, we listened to some Russian music by Winogradsky himself. He played the piano very well. The laboratory is now in good condition, and several microbiological problems are under way. We were very sorry to part from these people. We had so much in common with them. Next day, I went to Switzerland to fetch Byron. He was quite changed, spoke fluent French, with a Vaudois accent, and could hardly speak English.

September 28. We are now on a French boat on our return journey home. We had a rather rough sea yesterday and could hardly keep from getting sick. Byron is coughing badly and has to stay in bed. Later, I also became sick and had to spend nearly a whole day in the cabin. This, combined with a carbuncle on my neck, made me feel rather miserable. Later the weather improved, and the boat is making good time. Our trip will soon be at an end. On coming home, we will have to start looking for a house, take the furniture out of storage, and settle down to a routine form of life.

The great journey is coming to an end. On thinking this over again, I may add that the real journey is only beginning. It will only take a different form. First of all, I must organize and digest my many impressions, I must learn how to apply to my own work the many lessons that I have learned. I have seen a great deal and learned a great deal. Our trip to Russia was the climax of our whole journey. It exhausted us completely, both physically and spiritually. After what we saw there, there was nothing more to be seen, nothing that the human mind could grasp. We were saturated. We could thus pass through Amsterdam and Rotterdam without any desire whatsoever to stop and see these great cultural centers. We spent two hours at the station in The Hague, but had no energy or desire left to see that city. Even when we returned to Paris, we were no longer interested in seeing the many museums that we had not seen before. To what use? What new could we now add? Not only were we full of impressions, but these have now taken a new turn, a new light, a new interpretation, a new meaning. I could now rightfully say that I have seen all that a human eye could see, that I have heard all that a human ear could hear. Either there is a limit to what one is able to absorb, or certain events may occur in one's life, after which nothing else matters. Thus we are approaching our home shores, two weary travelers. We went abroad, like two innocents, anxious to see it all, to hear it all. Now we are returning. What use can we make of all that we have seen? Will it help me in my further work? Will it make our future life better, more interesting?

Postscript. The next few months after our return home were a difficult and busy period for us. We had to rest from our trip, summarize my scientific impressions of soil microbiology. These formed the basis for a comprehensive review entitled "Soil Microbiology in 1924: An

Attempt at an Analysis and at a Synthesis." I drew attention to the low state of this science in certain countries, notably in Germany and the United States. I emphasized: "The time has come to recognize that we are dealing with one of the most complex sciences, which depends, for its advance, upon a number of other sciences, especially organic, physical, and biological chemistry, and microbiology; that a thorough training is necessary in the most fundamental sciences before an approach can be made to this science; that it is important to keep in mind the whole complexity of the soil population and the numerous processes carried on in the soil; that the contributions made by students and practical men can be only subsidiary to those carried on by investigators who devote their whole life to the science. This is recognized in many institutions in Europe, but not by the great majority of the other investigators, especially in this country."

Broadening Horizons

I RETURNED from the grand scientific tour greatly enriched in knowledge and new ideas. After discussing my problems with masters in the field of microbiology, I found my future plans for the development of my own selected field of science, that of soil microbiology, gradually becoming clarified.

My future studies of the microbiological population of the soil and its role in soil processes, of the part played by fungi and other microorganisms in the transformation of organic matter in nature, and of the phenomena of associative and antagonistic interrelations among microorganisms were to benefit greatly. I also came definitely to the conclusion that there was a great need for a comprehensive treatise on soil microbiology, which would present a critical summary of the subject. What was needed was a source book and a guide for students specializing in this field, a book that would also serve for supplementary reading by all those interested in soil processes that depend largely upon microbiological activities and that are responsible for soil fertility. I decided to undertake at once the preparation of such a book, especially since I had already made extensive notes on the subject in connection with the lectures that I gave annually for several years to groups of graduate students.

The book would offer an understanding of the microbiological population of the soil, far more than a mere familiarity with certain groups of microorganisms inhabiting the soil, or with certain specific processes, no matter how important for soil fertility. This book should also deal with the complex interrelationships that exist among the numerous

microorganisms that inhabit the soil, as well as with the effects of microorganisms upon higher forms of life. After all, I well recognized that these organisms do not live in the soil in pigeonhole compartments, or as pure cultures. This is true of disease-producing organisms that live in the cells and tissues of animals and man, of plants and insects; it is likewise true of organisms capable of bringing about certain important industrial fermentations, as well as the fermentations of bread, wine, or beer. In the soil, however, the microorganisms live as complex populations. The fertility is controlled largely by their activities.

The major task toward which my own work was gradually to lead was to untangle some of these complex relationships, in order to understand the sum total of the microbiological processes in the soil as a whole. If I were to summarize briefly the types of investigations that were now gradually taking shape, first as plans on paper or as mental pictures, which were later to serve as a basis for detailed studies, I could only draw attention to the following investigations, which were soon started or contemplated:

1. The microbiological analysis of soils as an index of fertility; this was to lead me to the study of the complexity of the soil microbiological population, and of the various processes which result from the activities of its members.
2. The decomposition of plant and animal residues by microorganisms.
3. The problems involved in the production of composts, first of stable manures, then of artificial manures.
4. The origin and nature of humus in soils, in peat bogs, and in composts.
5. The effects of pure and mixed cultures of microorganisms on the decomposition of organic materials, notably proteins and amino acids, cellulose and hemicelluloses, lignins and starches.
6. The study of the effects of different organisms upon one another, their associations and antagonisms, which were finally to result in the development of the subject of antibiotics.

My decision to bring together for publication the material needed for a comprehensive treatise on the subject of soil microbiology was a result of several other factors that may be briefly summarized here:

(a) Some of the graduate students who continued to come to my laboratory in ever-increasing numbers from various states in this country, and soon from many foreign countries as well, served as a major stimulus for the preparation of such a book; the lectures and laboratory demonstrations which I had repeated for several years and which I had revised each year presented suitable material to serve as a basis for the preparation of such a treatise.

(b) Some of my students were gradually filling positions as teachers in various universities; they needed suitable sources of information for their own lectures and for their own investigations. They urged me, therefore, to proceed with the publication of my book.

(c) The available texts on the subject had been written either in German or in French; some of them were lacking, at least as I saw it, in a proper and logical presentation of the subject, or they were totally uncritical of the information they presented; most of them were very much outdated, or represented largely bibliographic compilations; they were either too elementary or covered only very few angles of the subject under consideration; at best they were based upon the time-honored processes of ammonification, nitrification, and nitrogen fixation, which I could hardly accept without a thorough reinterpretation and re-evaluation.

This book, which I tentatively called *Principles of Soil Microbiology*, was finished in 1926. I submitted the manuscript to several publishers. The response was unanimous: such a book would never do; it was too large, comprising about one thousand printed pages; it would never sell more than about five hundred copies, since only libraries would buy it; it would, therefore, prove a complete financial loss. Who would ever be sufficiently interested in such an obscure subject as soil microbiology as to justify the publication of such an extensive treatise? The whole idea appeared to the publishers as utterly fantastic. What was soil microbiology anyway? It was neither soil nor bacteriology nor mycology nor protozoology. The publishers suggested to me that if I would be willing to cut the size of the book in half, some consideration might be given to it. Even Dr. Lipman was inclined to view the matter of publication in that light. Why not make a proper textbook of it?

I could not agree to these suggestions. After having spent several years in preparing this volume, especially after having recognized, as a

result of my European journey, that there was a great need for a book of this nature, after having great hopes that this volume would become a monumental contribution to the subject to which I was devoting my life, I would hardly want to chop it down, to make an ordinary textbook out of it, for the mere reason that it would not sell otherwise, that it might prove an obligation rather than a source of income to a publisher. To publish the book in its present form, I would have been only too happy to mortgage the house that I had bought recently, had that house not carried so large a mortgage that it would have been impossible to borrow further funds with it as security.

I finally turned to the Williams & Wilkins Co., of Baltimore, who the year before had published the book on enzymes. That company had a much broader vision than the others and certainly more courage and initiative. They were willing to undertake the publication of my book in its present form. They were skeptical, however, of their being able to sell a full edition of fifteen hundred copies, and unless they could do that, they could hardly expect to cover their expenses. It would certainly require ten years, or possibly more, to sell out such an edition. Fortunately, they were willing to take a chance. To be able to sell the book at all, it would have to be sold at a reasonable price of ten dollars. One of the conditions they laid down was that I would receive no royalties for the first six hundred copies sold. To this, I heartily agreed. Further, when the proof began to arrive, I found that numerous changes and corrections were required, with the result that when I finished reading the proof, I received a bill for seven hundred and fifty dollars for author's alterations. The company was willing to wait for payment of this bill until the royalties, if any, began to accumulate. This would mean that the whole edition would have to be sold out before my indebtedness would be clear.

Fortunately, all my hopes and the expectations of the company were fully justified. The efforts that I had made in the preparation and publication of this book were not in vain. It was received with enthusiasm by both readers and reviewers throughout the scientific world. It was called *The Bible of Soil Microbiology*.

The book appeared in 1927. Within less than four years, the complete edition was sold out. A new edition was required. I took full advantage of the rapid progress that was now being made in this subject,

and of many minor and even some major errors that were overlooked in the first edition. It gave me an opportunity to review again the whole field of soil microbiology quite thoroughly. A second edition appeared early in 1932. This edition, as well, was sold out within the next few years and was out of print before the decade was over. There arose a great demand for a third edition. Unfortunately, this came at a time when my energies were directed elsewhere and I could find no time to re-edit the book, especially since the whole subject had undergone many changes in the meantime. Perhaps only my point of view had changed, but I could hardly allow the reprinting of a book which I knew was partly out of date. This book has filled a definite need. It may have been a highly important contribution made during this particular period to the subject of microbiology, as some reviewers said of it. But I felt that others might take up the problem of supplying a necessary book, and thus give opportunity to another point of view.

As a result of our ever-broadening program on the microbiological population of the soil and its role in the transformation of elements in nature, many foreign and an ever-increasing number of American students and visitors began coming to our laboratory. Many of them brought new points of view and thus helped to broaden the scope of microbiology in general and of soil microbiology in particular. I should like to refer to several of these students and collaborators, whom I have selected from the many, not because of their particular pre-eminence in the chosen field, but primarily to emphasize the scope of their interests, the variety of problems in soil microbiology that they represented, and my own, frequently only limited, contribution in helping to mold them as future leaders in their respective fields.

Harold Sandon came from England in 1925 to study the relationship of protozoa to bacteria in American soils. His coming to us was a result of an exchange arrangement between the Rothamsted Station and our laboratory, as discussed previously. Sandon was an industrious and capable person, so that at the end of his stay in our laboratory, he felt justified in applying for a Ph.D. degree at Rutgers University. When one of the professors at Rutgers asked him just why he was interested in receiving a degree here, when he could have easily obtained such an advanced degree from either Cambridge or London universities, which, in the world of science, would mean far more than a de-

gree from Rutgers, he received an immediate and rather pointed reply: "To be sure, you are right, but a degree from the Soil Microbiology Department of the New Jersey Agricultural Experiment Station is worth far more to me than a degree from either of the British universities that you have mentioned." Sandon made a detailed study of the protozoan population of New Jersey soils and came to the conclusion that no fundamental difference existed between American and English soils. This aspect of the problem was thus removed as a potential explanation for the discrepancies in the results obtained in the two laboratories. He further helped to clarify our respective ideas concerning the significance of protozoa in soil fertility. In our turn, we helped him to gain recognition of the importance of other groups of microorganisms, notably fungi and actinomycetes, in soil processes. Upon his return to England, Sandon continued his work at Rothamsted, and later became a professor in Egypt, and then in one of the other African universities.

René Dubos was a young Frenchman who after being graduated from the Agronomical Institute in Paris had spent a year in Rome at the International Institute of Agriculture. He came to our laboratory in 1924. Here he worked for three years, largely on the problem of cellulose decomposition by bacteria. At first, he took only a passing interest in the subject, and I began to wonder whether this was his true field of study. During the latter part of his second year, however, I came to the laboratory one morning at an hour which was still too early for his usual arrival. As I entered the incubator room adjoining the laboratory, I found him examining a bacterial culture, with his eyes shining; he was lacking words to express his amazement at what he saw. I recognized then that he was well on the way to becoming a scientist. His interest in the subject of microbiology grew rapidly. He finished his work in 1927 and received his Ph.D. degree from Rutgers. We recommended him to Dr. O. T. Avery of the Rockefeller Institute in New York, who was anxious to obtain the services of a promising young microbiologist, familiar with methods of handling soil problems, and who would be qualified to help him in the isolation of bacteria capable of destroying the capsular substance of the pneumococcus organism. Little did I dream at that time that these studies were to lead Dubos to the solution, in a most brilliant manner, of this and certain other important problems in the field of microbiology, and would result in the

isolation from the soil of an organism which would produce an anti-bacterial substance, tyrothricin, which in its turn was to influence my own future work on antibiotics.

Elias Melin came to the laboratory, in 1927, from Sweden. He was already recognized as a brilliant investigator of the mycorrhiza fungi of forest trees. But before he could elucidate the role of these organisms in the nutrition of trees and in the problem of forest vegetation, he had to gain a better knowledge of the decomposition of organic matter in forest soils, of the nature of humus formation, and of the part played by the fungi in humus decomposition. With this in view, he selected our laboratory, where he spent a year. He familiarized himself rapidly with the various procedures that we had developed for the study of the composition and decomposition of plant residues in the soil, and was successful in carrying out a comprehensive investigation of different types of forest litter. Upon his return to Sweden, he entered into a severe competition for the Carl Linneus Professorship of Botany at Uppsala University. His training in experimental botany and the knowledge that he had gained in the study of nutritional problems of plants helped him to become the leading candidate for this position, which was awarded to him. In this case, as well, the student not only benefited from the teacher, but the teacher learned much from the student, especially about the ecological approach to the problem of the soil-inhabiting lower plants. My relations with Melin, like my relations with many of my other students and collaborators preceding and following him, fully illustrated the old adage that it is sometimes difficult to say whether a student learns more from his teacher or a teacher from his student.

Hesselink van Suchtelen was a Hollander by birth and a former student of the soil bacteriologist Alfred Koch in Göttingen. Although he took his degree in Germany in 1910, the year that I came to the United States, he was unable to find a suitable position in soil microbiology, for which there was little recognition at that time. He spent some time in this country teaching microbiology, but he returned to Europe soon after World War I. There he became connected with an industrial laboratory in Germany. He gradually became tired of this work and decided to return to soil microbiology, his first love. To familiarize himself with recent developments in the field, he made

arrangements to come to our laboratory, in 1930. When, upon his arrival at New Brunswick, he first saw me driving an automobile, he expressed great joy at having lived long enough to find at least one person who was capable of making a living out of soil microbiology, and, even more, rich enough to be able to buy a car. He was accustomed to the point of view prevalent in many European countries and also in America that this field of research must be considered a luxury science, which at best could only explain certain soil practices, and not a fundamental science which could contribute new interpretations of natural processes and which might yield new practical applications. Van Suchtelen was a philosopher and had a thorough understanding of the historical background of soil microbiology. In our laboratory, he devoted his time very diligently to the study of the thermodynamics of the soil, a problem closely associated with his interest in the decomposition of soil organic matter and the evolution of carbon dioxide. He became my close friend, with whom I could freely discuss the various problems that were disturbing me at that time. He encouraged me to travel to Europe as often as possible to come in contact with other investigators in my chosen field. His favorite expression was: "Even a fountain must dry up if it does not receive new sources from outside."

There were many other investigators who spent varying periods in our laboratory, some receiving advanced degrees from Rutgers, some not; some making important contributions to microbiology during their stay, and others familiarizing themselves largely with methods we had developed. I should like to mention here F. C. Gerretsen from Groningen, Holland, who came to study the principles underlying the making of composts. Dr. Jacob Blom and Mr. Tovborg Jensen came from Denmark; they worked, respectively, on problems of nitrogen fixation by soil bacteria, and on the effect of soil reaction upon microbiological processes; both returned to their native country to occupy important industrial and university positions. Hans Jenny came from Switzerland to study organic matter decomposition; he remained in this country, becoming Professor of Soil Chemistry at the University of Missouri and later of California. Walter Niessen and Fritz Schaffer came from Germany, to spend varying periods of time studying organic-matter decomposition. Walter Kubiena came from Austria

to study the microbiological population of the soil by cultural procedures, as compared with the results obtained by the use of a special soil microscope which he himself had developed. Some students and investigators came from Russia, others from Belgium and other European countries. A number came from Japan, China, and India. The further scientific progress of these investigators, after they left our laboratory, depended greatly on the particular individuals, their previous preparation, and subsequent opportunities in their respective countries.

Many of my students became associates and close friends. Some of them now occupy important positions in industry, in universities, or in government research. I need mention here only a few, to illustrate the broad scope of their problems and the variety of occupations in which they later engaged.

Robert L. Starkey came to our laboratory in 1921, after he was graduated from Massachusetts State College. He devoted his time here to the study of decomposition of organic matter, partial sterilization of soil, and sulfur oxidation by bacteria. Upon receiving his Ph.D. degree from Rutgers, he accepted a position at the University of Minnesota, where he made an important contribution in developing a course in soil microbiology and in initiating a research project on iron oxidation. When, in 1927, I was allowed additional funds by the Experiment Station to appoint a full-time associate, I called him back to our laboratory. He has remained here since and has become my close collaborator as well as a constant friend, with whom I have discussed freely my problems, and with whom I have shared my joys and sorrows. He has initiated some highly original problems in the field of microbiology which were largely concerned with the oxidation and reduction of sulfur by bacteria, the corrosion of steel, and a variety of microbiological changes in the soil.

Among my many other American students and collaborators were Robert V. Allison, who later became director of the Peat Experiment Station in Florida; Melville C. Allen, who became research specialist for the National Biscuit Co.; Robert A. Diehm, who became bacteriologist at Rhom & Haas Co. and later director of research for a container corporation; Charles H. Renn, who became professor of sanitary biology at Johns Hopkins University; Jackson W. Foster, who became bac-

teriolgologist at Merck & Co., and later professor of bacteriology at the University of Texas; Charles Skinner, who became professor at the University of Minnesota, and later head of the Department of Bacteriology at the University of Washington; H. B. Woodruff, who became chief of the Bacteriology Division at Merck & Co. There were numerous others. They made possible the many investigations in which I was engaged. Some of them were responsible for the formulation of new ideas. At all times, they were a source of constant encouragement and a stimulus for further research.

I have thus listed three aspects of my scientific activities during the first period of my work at the New Jersey Experiment Station: my scientific work itself, the books I have written, and my teaching and students. There remains, however, a fourth phase, which is just as intimately connected with my scientific progress as any of these. This comprises the various scientific congresses, national and international, which gave me the opportunity to meet outstanding investigators, not only in my chosen field of soil microbiology, but also in allied fields, notably pedology, soil chemistry, and plant nutrition, on the one hand, and bacteriology, mycology, and biochemistry, on the other.

The First International Congress of Soil Science was held, in 1927, in Washington. The congress was followed by an extensive excursion, lasting a month, through the United States and Canada, to examine various American soil types. Dr. Lipman was president of this congress. I took an active part in the organization of the program of the congress as a whole and of the Commission on Soil Microbiology in particular. In the absence of Dr. Stoklasa, who had been elected in Rome as president of this commission, I presided in the capacity of vice-president. At the end of the Washington congress, I was elected president of the commission. The meetings lasted ten days. They afforded me further opportunities to discuss various problems in my selected branch of science with some of the outstanding investigators in this and allied fields. As a result of the group and private discussions held during the long trip across the country, and following the examinations of the numerous soil profiles dug up throughout the broad acres of North America, from North Carolina to southern California and from southern Georgia to western Canada, I learned to understand better the nature and origin of the soil, the geological, biological,

and chemical processes that have contributed to its formation, and the potential utilization of different soils for agricultural and other purposes. I could thus better visualize the role of the microbes in the processes of soil formation and transformation, with special bearing upon soil fertility and plant growth.

The second congress was to be held in the Soviet Union in 1930. Since I was one of the commission presidents, I had to prepare the program for this congress. In this connection, I welcomed the possibility of going to Europe in 1929. This gave me an opportunity to attend the meetings of the Commission on Soil Microbiology in Stockholm. The following year, in 1930, I attended the congress itself, which was held in Leningrad and in Moscow. These meetings further helped to establish and cement my international connections with various European investigators. They also contributed to the crystallization in my own mind of the prevailing concepts in soil microbiology. I could thereby learn to recognize the many gaps that existed at that time and attempt to answer some of the questions raised. To some of these questions there was as yet no answer.

Most important of these questions was: Is soil microbiology an independent science or is it merely an application of a certain amount of knowledge of microbiology to soil processes and to plant growth? A definite answer or at least a special attitude or point of view was required to decide where to go from there. Should one concentrate his interests on the study of the soil, its structure, its physical and chemical reactions, and determine how the microbes live, where they fit in the complex make-up of the soil, and especially how their activities influence soil structure, soil conservation, and the growth of higher plants, both cultivated and uncultivated? Or should one concentrate more on the microbes themselves, their nature, their distribution in the soil mass, their physiology and biochemistry, and their activities in the soil, resulting primarily from the microbiological, rather than the soil, approach? In all my previous investigations the pendulum had been swinging back and forth between these two approaches to the study of soil microorganisms. My work on the occurrence and nature of fungi and actinomycetes in the soil, on the sulfur bacteria and their metabolism, on the action of fungi and bacteria upon simple and complex organic substances, and finally their effect upon one another—

all these could definitely be classed with the second, or the microbiological, approach to the study of the soil. On the other hand, my work and that of my students on the partial sterilization of soil, on the methods for measuring the microbiological condition of the soil and its effect on fertility, and finally the many studies that we had made on the decomposition of plant residues in composts and in soil—all definitely reflected the soil point of view of microbiology. This pendulum continued to swing during the next two decades, in ever-widening arcs. At certain periods, notably before my interest in antibiotics, my major concern was with problems which might be considered as distinctly of soil interest, whereas at other periods, notably beginning in 1939, when the antibiotics program was developed on a full scale, my interests may be said to have been largely of a broad microbiological nature.

During my various travels to Europe, in visiting the many university, government, and industrial laboratories, there were numerous opportunities to consult with different investigators, who had special information, a special point of view, or even a special philosophy, and often all three. These had a direct or indirect bearing upon my own field of study. This is hardly the place to mention all these investigators. A few illustrations may suffice. As mentioned previously, Professor E. Abderhalden of Halle, the venerable biochemist and editor of several series of monumental volumes on biology and biochemistry, covering all the biological sciences from algology to zoology, requested me on two occasions to prepare summaries of methods used in soil microbiology. The first invitation resulted from a statement he made when I visited him in 1924 that he had invited Prof. Stoklasa of Czechoslovakia to prepare for one of his volumes such a section, asking my opinion of it. After examining Stoklasa's article, already in proof form, I found that it was inadequate, uncritical, and hardly covered the modern point of view of soil microbiology. When I informed him to that effect, Abderhalden invited me to prepare a parallel section. This I decided to do, after first discussing the question with various colleagues. We believed this to be highly desirable, in order to offer to the German-reading public a more critical attitude toward soil microbiology. Stoklasa was more at home at formal receptions and

banquets than in a microbiological laboratory; he appeared to depend largely upon secretaries or assistants to make his compilations, and not upon his own critical faculties, with the result that his books appeared to be nothing but summaries based upon abstracts published in various journals rather than upon original data and interpretations.

Prof. H. Niklas of Weihestephan, Bavaria, was what one might call a second edition of Stoklasa. He was looking around for a subject upon which to build his fame. He found it in the utilization of certain microbes for measuring soil fertility processes. Whatever the practical significance of the results of his work may have been, it tended to throw an unenviable reflection upon soil microbiology as a fundamental science. Had such procedures been recognized for what they were worth, of limited scope and application, no one would have questioned their use. Unfortunately, the impression was given that one was dealing here with a fundamental phase of soil microbiology, with the result that the question was often raised whether this field of knowledge was a science at all.

There were others, like Dr. Von Kreybig of Hungary, a charming gentleman, but as devoid of any clear concept of soil microbiology as only certain practical agronomists could be, and, I hasten to add, who would never have claimed to be a soil microbiologist had it not been for his efforts to build a reputation for himself by selecting this field of study. When we were in Budapest in 1929, attending a soil chemistry conference, he invited us to spend a week end on his estate, some fifty miles from that city. There we met with a charming reception in an atmosphere reminiscent of the feudal state of the Middle Ages. Early the next morning, I was invited by our host to visit his experimental farm. He confided in me that he had made a great discovery; he had developed a bacterial inoculum for his wheat fields which greatly increased the yield of his crops. A careful examination of his culture beds revealed that he had nothing but a "nitriary," as it was called during the Civil War in our own Southern States, for the production of nitrates. What he was feeding his wheat fields was far more than a culture of bacteria; it was a highly available source of nitrogen, in the form of nitrate, and probably also some potash and phosphorus. But he ascribed the favorable effect upon the crop to the

action of the bacterial inoculum. I tried, as politely as I could, since after all I was his guest, to point this out to him, but without convincing him. Perhaps he did not want to be convinced.

Many a time, I had to battle the imposter, the charlatan, and the plainly ignorant, who used the mantle of soil microbiology for his own unscrupulous purposes. Occasionally I won; more often I felt as if I had put on the cloak of Don Quixote and was battling windmills.

Fortunately, as a counterbalance to these rather painful experiences, I had ample opportunity to meet other investigators, who had a true scientific approach to the subject of microbiology as a whole and its application to the soil in particular. First place should be reserved for S. N. Winogradsky. This venerable scientist, whom I never failed to visit upon all my trips to Europe, always emphasized the fundamental approach to the subject of microbiology. He submitted to a highly critical analysis the superficial attitude of the many who attempted to use soil microbiology as a subject for self-glorification.

Next to Winogradsky in influencing my attitude toward the science of microbiology came W. Beijerinck. Although I spent only one day with him, as described earlier, this day left a profound impression upon my subsequent work. When Beijerinck retired (or was "forcibly removed," as he so bitterly expressed it) from his laboratory in Delft, his place was taken by Kluver, whom I had the good fortune to meet first in 1930, at the Botanical Congress in Cambridge, and later in his own laboratory and on various other occasions. Kluver did full justice to the science which he inherited from his great predecessor. His contributions and those of his many students to microbial physiology stand out as a landmark in the field of microbiology.

In Cambridge, England, I came to know a group of brilliant biochemists who had made important contributions to microbiology as a field of science and thus helped to place it on a par with other biological sciences. These included Marjorie Stephenson, who wrote a stimulating book on Microbial Metabolism, and others.

In this country, as well, my contacts with various workers in microbiology and allied fields continued to enlarge, some becoming highly fruitful. My friendship with Dr. Charles Thom took a new turn when he inherited the position in soil microbiology, at the Bureau of Plant Industry. Together with N. R. Smith and some of the younger as-

sistants, he undertook a series of investigations on organic-matter decomposition, on the fungus flora of the soil, on the bacterial population of the soil. I also came in frequent contact with Professor E. B. Fred, who, together with Dr. I. Baldwin and other assistants, was in the process of building up a new and important school of soil microbiologists at the University of Wisconsin. Most of their attention was devoted to the anaerobic bacteria and to the root-nodule organisms, and later to the whole problem of nitrogen fixation.

My own work on organic-matter decomposition and on the role of microbes in the transformation of nitrogen resulted in my receiving, in 1929, the Nitrate of Soda Nitrogen Research Award. This award carried with it a considerable sum of money, which I set aside to subsidize subsequent scientific trips to Europe. I was also asked to serve as chairman of the Committee of the American Society of Agronomy for making the award for the following year. This brought me in close contact with the members of that society during the next few years. I was made a fellow of the society, finally serving as vice-president for four years. I also continued to attend regularly the meetings of the Society of American Bacteriologists, where I frequently presented papers dealing with my work and with that of my students. I was later to become president of that society and during World War II served as chairman of an important committee. I was thus able in various ways to use my official contacts with both national and international societies to encourage general interest in soil microbiology.

As to my personal life during these formative years, certain special aspects may be noted. In 1925, I was made associate professor and in 1929 full professor at Rutgers; this resulted in gradual increases in my salary and living standards. In view of our frequent trips to Europe, the salary alone did not suffice. Fortunately, I was able to carry out a certain amount of consultation for some companies interested in the manufacture of enzymes. This type of work did not interfere with my regular duties at the Experiment Station and actually contributed to the development of a broader point of view of microorganisms and their utilization in industry.

We were able now to buy a modest house, which we gradually furnished. We were able occasionally to send our son Byron to a sum-

mer camp and take greater advantage of our proximity to New York to attend concerts, see plays, and meet friends. In New Brunswick itself our circle of friends was rather limited, in spite of the presence of the university and my official connection with it. We did not play bridge and had certain special interests which set us somewhat aside from other groups in the city. The provincial attitude of many of those with whom we should have been good friends, and the limited cultural outlook of those with whom we could have been friends had we cared to—all greatly reduced the number of those with whom we could associate freely. We, therefore, had to fall back more and more upon our friends in New York for supplying the need for social contact. Fortunately, the proximity to that city made this easy. We never became identified with any definite groups in New York, as could easily have been the case, since we never stayed there long enough. We picked our friends as individuals and not on the basis of their origin or group affiliation. At all times, my best friends were my students. We worked together, we shared our mutual interests, our dreams and our hopes, our present and our future.

Men and Microbes

MY VERY first attempts to start on a scientific career in search of microbes, the beneficial ones, especially those that occur in the soil and are responsible for most of the soil processes, brought me in close contact with other investigators, some of whom were sympathetic and others critical. When, in the fall of 1915, I succeeded in isolating a large number of molds from the soil, I was at a loss as to how to identify them or what to call them. There were many, notably the various penicillia, which had already become famous for the numerous cheese-ripening forms and later were to become even more famous for the forms that produce penicillin. How was I, inexperienced in the study of molds as a whole and of penicillia in particular, to recognize and determine their specific role in various natural processes? I could get little help from the botanists at Rutgers. Dr. Lipman then advised me to spend a few weeks with Dr. Charles Thom in Washington, a leading student of this group of microbes, who began to study molds because of their role in cheese making and ended by studying their occurrence and activities in the soil.

When I arrived on a bright September morning at his small laboratory in the old Department of Agriculture building in Washington, loaded with a satchel full of cultures, Dr. Thom greeted me cordially. He took one look at the cultures and said: "Look here, young man, you are not planning to spend a lifetime here. Close your bag and listen to what I am going to tell you." On his instructions, I visited the Washington fruit market the next morning and examined numerous oranges, lemons, and other citrus fruits. To the great disgust of

the dealers, I touched a number of moldy fruits, as I was instructed. That afternoon, I reported to Dr. Thom that, according to my observations, both by surface appearance and by sense of touch, there were two molds involved in the spoilage: one produced a soft rot and the other a hard rot; there was also a difference between the shade of green of the spores of the two molds. He looked at me quizzically and said, "You will do. Those two molds are *Penicillium italicum* and *Penicillium oxalicum*. Now, let us get to work." He took out the microscope and showed me the exact difference in the structure of the two molds.

For the next two or three weeks, Dr. Thom gave up all his other work and devoted his time to me, examining a number of the cultures that I had brought with me and had isolated from various soils and a number from his own collection. Puritanic in nature, strict in his relations with people, he was a true scientist. There was nothing superficial about him. He was willing to battle for an idea that he believed was sound and for the man whom he believed to be honest. I spent many an hour with him, listening rapturously to his discussions about molds, about people, of whom he was for the most part highly critical, and about scientific developments in this country and abroad.

There were other striking personalities in my early scientific career. There was Charles Lipman, Professor of Soil Bacteriology at the University of California. A pleasant and sincere person, an excellent teacher. His scientific work, however, did not seem to lead him anywhere. Having reached blind alleys in every approach he made to soil microbes, Charles Lipman turned to fantastic subjects such as finding bacteria in ancient earths and in meteors. His methods were open to criticism and his results signified little. There was, of course, some truth in what he said, but he lacked the courage, imagination, or preparation to open up new paths for the solution of difficult problems.

It was my first European journey in 1924 that made it possible for me to come in contact with a large number of outstanding investigators. This voyage was devoted largely to a search for basic facts, for scientific principles, especially in the field of microbiology as it applies to the soil. I discussed my problems with chemists, bacteriologists, botanists, and others who had either fundamental knowledge or practical information. Many of them helped me clarify my own vague

ideas, germinating concepts, complex interrelations. Others supplied me with specific information and critical approaches, so that I could appreciate better what had been done before, what should be done now, and what were the prospects for the future in my own selected field. A few tried to confuse me; my attempts to convince them helped sharpen my own wits!

I found stimulating minds everywhere, independent of the size or nature of the country, of the size of the laboratory or the number of collaborators, of the field of science or its practical application. I tried to present my own interests everywhere I could. Often I was rebuffed, but more often I was received graciously and listened to attentively. Usually without proper letters of introduction, I would knock at the door of a famous scientist. Once I would gain admission, the interview would last much longer than one would anticipate. Only twice was admission refused me on the basis of improper introduction, but even in these cases I was able to impress the person involved, once I met him for a minute or two, that my problem was important and a more prolonged interview was in order.

In Paris I met Professor Schloesing of the Institute of Applied Chemistry, the son of a famous soil chemist and himself an outstanding investigator. Here was Gallic wit and Gallic temperament. You did not have to ask many questions. He knew them before you asked them, and perhaps could state them better than you could yourself. Yes, his father worked on nitrification as far back as 1875-1877, he himself worked on nitrogen fixation by legumes way back in 1892. He followed with much attention the progress of soil microbiology. It was too difficult, too complex. He liked simple, isolated reactions, preferably of a purely chemical nature. He was concerned least with complex populations, in which nobody is sure what any one microbe is doing at a given time. No, thank you, you were welcome to waste your time at it. Not he. He had had enough. One thing today, another tomorrow. Bacteria now, fungi later. And what do you call them? Actinomycetes? Nobody even knows what these things are, let alone what they do. Perhaps there is something to these complex interrelations, to these effects of one organism upon another. So what about it? How does it all help to understand the liberation of nutrients in the soil, the rate of growth of crops. Even the simple processes, like

nitrification, are difficult enough. Had not nearly fifteen years passed between the time his father demonstrated the biological nature of the process and the time Winogradsky isolated the causative organisms? Look at these legume bacteria that produce nodules on the roots of clover, alfalfa, or other leguminous plants. What do we know about them? No doubt they are tremendously important, since they exert such an important effect upon crop yields and are responsible for such considerable increases in our food production. But we still do not understand the mechanism of the fixation process. How is it that these legumes can use the gaseous nitrogen in the atmosphere, whereas wheat, barley, and other crops are unable to? Incidentally, is it the plant that fixes the nitrogen, or the bacteria? Why is it necessary for both of them to live together (symbiosis) to accomplish this? What part does each play? Here is a complicated enough problem for you. Why dig deep into these numerous interrelations among thousands of organisms?

He was brimming over with ideas. Here was a unique, logical mind, totally different from anything I had ever seen before. He overwhelmed me. I was full of admiration for him and what he stood for. I kept asking further questions, about science in general, about microbiology in particular, about French scientists and their contributions, about the limited work being done now. He answered frankly and in detail everything I wanted to know. He also regretted that the subject of soil microbiology was so much in disrepute in France, a country which had contributed so many great agricultural chemists and microbiologists.

Dr. Gabriel Bertrand was a totally different personality. A great chemist, whose work on trace elements in the soil was epoch-making and who made important contributions to our knowledge of the chemistry of various plant constituents. Sitting in his office dressed in formal black attire, he received me. He did not offer me a chair and seemed to be wondering who I was to waste his precious time. He answered my questions coldly and precisely, and did not seem to show much interest in what I had to say. Definitely, he was not interested in microbiology. As I turned to leave, I finally asked what he thought of Pringsheim's ideas about the structure of the cellulose molecule. That was the crucial question. It aroused him fully. He became quite

emphatic in his denunciation of Pringsheim's ideas. Finally, something dawned on him. Well, what is your interest in cellulose? Its decomposition by bacteria? Why didn't you say so? I have a student who is doing something that will be of distinct interest to you. He became very cordial, led me to the laboratory, introduced me to his student, Miss Khouvine, who was isolating cellulose-decomposing bacteria from human wastes. Further discussion became animated, and I left with a fine impression of excellent scientific work.

We were received with open arms by other scientific workers in France, notably Demolon at Versailles. He and his wife, a native of Georgia who spoke a delightful Russian, proved to be most charming hosts during the day we spent in Versailles. We were shown the palaces and, more important, the experimental gardens and laboratories where Demolon had been working on the problem of nitrogen fixation by legumes. This is an important practical question, especially for some of the impoverished soils in France. Demolon himself spoke good English. He was wounded in the war, and, possibly because of that, developed a sharp, critical attitude toward certain scientists, notably Germans or those under the German influence. He was a fine scientist, with a good understanding of the agricultural needs of France and of means required for improvement. His thin, drawn face and the dark glasses he had worn since his eyes were injured in the war tended to give him a certain Mephistophelian appearance, hiding a charming personality.

But it was Winogradsky, that great master in the field of microbiology, especially in my own selected branch of the subject, whom I came to respect and admire, both as a scientist and as a man. We carried out an extensive correspondence on the subject of soil microbiology. He was sixty-eight years old when I first saw him and ninety-four when I saw him last. He had a striking personality. Winogradsky was of medium stature, sparsely built, with a thin crop of grayish hair. He spoke beautiful French, with the fine gesticulating hands of the artist that he was. He would always get down to the root of a problem. He had no patience with the laggard, the superficial investigator, the generalizer on the basis of few facts. He was, therefore, highly critical of other investigators, but was always eager to welcome a novice. He would ask me anxiously: "Don't you think he has something?" Or, he

would comment, "He is like a stallion eager to go, but we must watch that he doesn't break through the traces."

Winogradsky lived long enough to bring some new ideas to soil microbiology. He spent the years of the second World War cut off in his laboratory from the rest of the world, bringing together all his scientific work of fifty years, so that it could be published while he was still alive. It took two special trips to Paris on my part, in 1946 and 1947, to make arrangements for this publication. It also took a lot of work to convince the National Academy of Sciences in this country to provide the necessary paper and supplementary funds to make possible this publication, since the Pasteur Institute and the publishing house were short of both.

The other great master of general microbiology, Wilhelm Beijerinck, was a personality similar in many respects to Winogradsky but strikingly different in others. Austere in appearance, strict in his attitude toward others, apparently lacking in artistic or literary interests, his whole life was devoted to science. Every word he uttered was along scientific lines. He was critical of Winogradsky because the latter did not have to depend upon a salary for his living. He belonged to the group of pioneers who opened up new fields of science, who pointed to new horizons, not exhausting each subject themselves but leaving much for others to follow, those who attracted and encouraged many students and who left a great school. Beijerinck definitely belonged here.

Here were two outstanding personalities, both of whom greatly influenced my own work, at whose doors I knocked with hat in hand, asking for guidance. I met many others. There was the brilliant Svante Arrhenius, of Stockholm, the immunochemist, who was already entering the declining years of life and scientific productivity. I was introduced to him by his son Olaf. On his way to a reception at the American Embassy, he received me in his frock coat. His full, red face, almost apoplectic, betrayed good living and especially heavy drinking. Although in his own work he barely touched the field of microbiology, he was intensely interested in what I proposed to do and showed keen regret that it was no longer possible for him to become engaged in a new field.

Another striking personality was Prof. W. Kruse, famous bacteri-

ologist at the University of Leipzig. Before entering his office, I was submitted to a detailed interrogation by a kind of drill sergeant in the outer office. When Kruse was finally informed who I was and that I insisted on seeing him, he came out of his laboratory to converse with me for five minutes. He never asked me inside. His thin face was covered with a grayish, scrawny beard, which shook every time he raised his head. He reminded me so much of our town goat in Priluka that I could not help but smile in the midst of our serious conversation. In my graduate student days I had greatly admired his book *Allgemeine Mikrobiologie* and expressed regret that he was not bringing it up to date. He kept shaking his head, repeating: "It is too late for me, it will be up to you younger people to do it."

A totally different impression was left by the brilliant biochemist, R. Willstätter, whom I visited a few days later in Munich. Although a line of students was waiting to see him, he received me as soon as I was announced. He answered all my questions and showed keen interest in my plans, especially those bearing on composition and decomposition of plants. I was interested in his work on chlorophyll, his training and subsequent association with the famous chemist Von Beyer, whose place he took in Munich. This was a laboratory with a well-deserved reputation for continuity in the study of organic chemistry, begun with such famous investigators as Liebig, whose work on human and plant nutrition influenced greatly my own work in this field, continued by Von Beyer and Willstätter, and more recently by Wieland. I found Willstätter to be one of the few scientists in Germany, if not in Europe, who had a very high opinion of the work of American chemists and of American investigators in general. His pessimistic view of life was that human beings, always the same, never learn any lessons; that the technics may be improved and the externals of life changed, but man always remains the same, the fighting animal. Enriched in knowledge but depressed by his pessimism, I left him.

Among the other brilliant investigators who have influenced me profoundly and whose friendship and support I valued very highly, I need only mention Orla-Jensen in Denmark and Barthel in Sweden. The first was concerned primarily with lactic acid bacteria and their growth. He first demonstrated the importance of vitamins in bacterial nutrition. His ideas on bacterial classification greatly influenced sub-

sequent developments in the field. His concepts of the role of microbiological processes in nature were as sound and as solid as his native land and its agricultural produce. From the time I first met him in his laboratory in 1924 until I parted from him nearly a quarter of a century later at the International Microbiological Congress in Copenhagen, I found great moral strength in him and excellent guidance in many problems related to his field of interest. His home was always open to us whenever we came to his native land and his advice always freely given.

Prof. Barthel's friendship, lasting for more than thirty years, I valued very highly. From the time he first visited my laboratory in 1922 and saw my early work on partial sterilization of soil until December, 1952, when he came to congratulate me upon the award of the Nobel Prize, our paths crossed constantly, certainly to my great advantage. When we embraced at our last meeting to say good-by, there were tears in our eyes. Mine were tears of gratitude for having enjoyed such friendship all these years from a good man, a good scientist and devoted friend.

Need I mention others? There were many, but their influence was less felt, perhaps less lasting.

These men helped me to clear up the existing confusion in my own mind concerning the microbes, their activities, and their role in natural processes. They helped me to synthesize observations made in the laboratory and those obtained from extensive reading of the work of older investigators. Here was a complicated subject which dealt with a complex microbiological population, inhabiting a highly complex natural medium, the soil, the earth under our feet. What do these microbes do? How do their activities influence one another? What are the resulting products? How do these influence the growth of plants, of animals? What about the waste products? What about their role, individually or collectively, in the cycle of life as a whole, and in the transformation of individual elements in particular? Were the processes in the sea the same as in the soil? If they were different, why? Was it due to differences in the nature of the microbes, or to the natural substrate in which they lived?

I directed these and other questions to anyone who was willing to listen to me, and often only to myself. The answers were not always

satisfactory. They usually led to other questions. As I analyzed the answers, as I correlated them with the actual observations, I was not always satisfied that I understood clearly enough to envisage the solution to the problem. More often, I was more impressed by what we do not know than by what we know already. This enabled me, however, to go back to the microbes with the questions thus formulated in my mind. I isolated cultures of various microbes from the soil or from the compost, from the sea or from the peat bog. I grew them on special media. I examined them under the microscope. I modified the chemical composition of the medium, so as to influence their growth and their activities. The microbes did not deceive me. They gave me the answers.

Frequently such answers led to other experiments. Occasionally they led to new fields of investigation. But invariably, the answers were there for anyone who wanted to come and interpret them, to take advantage of them. Some of them were spectacular, as in the case of the elucidation of the origin and nature of humus, a subject discussed in detail in the next chapter, or as in the formation of antibiotics by actinomycetes, which will be discussed later.

The Humus Period

ALL my efforts in the field of science thus far had been focused on the microbiological population of the soil, its nature and activities, and especially its role in soil fertility, specifically the decomposition of human, animal, and plant wastes.

I was now approaching a period in life which usually proves to be the most fruitful and which should normally have become the pinnacle of my career. Had this been the last period of my scientific career, had it led to a gradual consolidation of my life's work, I would have been fully justified in looking back upon my own contribution to the cycle of life in nature, especially to the role of microbes in this cycle, as fruitful if not revolutionary. I probably would have been able to say that this was the period of my greatest scientific activity, that it was the culminating point of all my previous preparation, of all my plans and hopes. Although during this period I devoted considerable time to certain phases of microbiology, most of my efforts and those of my many students and collaborators were closely bound with the problem of humus, its nature, formation and decomposition, in soils and in composts, in seas and in peat bogs. It was this group of problems that gave color to nearly the whole decade of my scientific activities from 1929 to 1939.

During this period, I examined numerous peat bogs in this country from Maine to Florida, in Europe from Sweden to Switzerland, and in Palestine. I studied composts of various types ranging from those of stable manures to artificial composts, for mushroom production and for soil improvement. I examined humus in various soil types,

from those of the cold north to those of the hot southern climates, from those of different forests and alpine peaks to those of wastelands. I studied organic matter in sea waters and in sea bottoms. It appeared that there was not a form of organic matter undergoing decomposition under natural conditions that failed to receive some consideration. In all these studies, I was guided by a single purpose: to establish how humus is formed, what it is, and how it is decomposed further. I even ventured to touch upon the relation of humus to coal formation, on the one hand, and to oil-bearing strata, on the other. Everywhere, I tried to establish the role of the microbes in these processes.

In this work, I was assisted by many collaborators from all over the world. These studies resulted in many publications. First came a comprehensive volume on *Humus*, published first in 1936 and followed two years later by an enlarged second edition. A number of scientific papers, addresses, and reviews were published in various journals throughout the world. As a result of our work on peat formations, the State of New Jersey extended an invitation to make a detailed survey of its peat resources, which led to the publication of two volumes. In collaboration with my associate, Dr. Starkey, I published a textbook entitled *The Soil and the Microbe*, which proved to be useful, for nearly twenty years, when it was superseded by another book on *Soil Microbiology*.

During this period, I made five more journeys to Europe, in 1929, 1930, 1933, 1935, 1938. I visited nearly every country of major scientific importance and attended a number of international conferences, largely devoted to soils, to plants, and to microbes. Although my interests dealt chiefly with the subject of formation and decomposition of various natural organic formations, or humus types, always in the background was my first love, a better knowledge of the nature and activities of the microbes. Wherever I could, I talked about these microbes and looked for them. In Finland and Sweden, in Palestine and Egypt, in Russia, in England, and in the United States, everywhere, I looked for microbial populations. I dug into peat bogs and into forest soils, I smelled them, I analyzed them, I talked about them, and thus gradually learned a great deal about their nature and formation, and about the activities of the ever-present microbes.

No wonder then that when I was invited in 1938 to go to the

Holy Land to evaluate a certain peat formation that was there when the Bible was first written, where the river Jordan begins and where the wild papyrus grows, north of the Sea of Galilee, I felt confident enough to accept the invitation and to express an opinion which appeared to be somewhat at variance with those presented by British engineers and German chemists. My opinion was based upon the facts of the situation as I saw it, since I had learned by that time that the nature of peat is governed by the geography of the region, its topography, the prevailing climate, the past and present vegetation. Its utilization would have to be controlled, therefore, by all these factors as well as the local needs for sources of organic matter. Unless this was well understood, one was apt to ruin a peat bog, and in the Holy Land, this bog was the only one available.

When my observations of this peat led me to report that this type of material was no good as a source of fuel, that it was no good for use as litter in stables, many of the German refugees who had settled in the land and who knew from their past experience only the moss peat type thought that I was ignorant or even prejudiced against their interests.

I had many other experiences which resulted from my studies of organic matter in nature, some spectacular and others less so, but all interesting or exciting. I would like to mention a few here. They will illustrate how fundamental facts once established will lead, sooner or later, to practical applications.

One Wall Street magnate, attempting to extract riches from a peat bog in Florida, learned to his dismay that peat represents a group of problems that cannot be solved from behind a mahogany desk. Too late for practical purposes, he appealed to me, in 1938, for a copy of my *Humus* book, which would enable him to familiarize himself with problems that he should have recognized before embarking on an extensive program of exploitation of a natural peat bog.

When, on one of my trips to northern Maine, I found that some of the bogs in that region would yield a type of peat in no way inferior to the imported peat moss, I decided that here I had a weapon, however small, whereby I could fight Hitler and his German cohorts by making my adopted country independent in its supply of peat moss.

These studies led to the development of two bogs for practical, large-scale production of such peat.

In 1929, I went to Florida, at the invitation of a banking group, to advise them concerning the value of a certain peat formation in the northern part of that state for the growing of sugar cane. Various studies on the nature and decomposition of that peat convinced me that, if judiciously handled, it could be made to last almost indefinitely. I advised the group accordingly. I also carried on an extensive correspondence with numerous investigators throughout the world, as well as with chemists and engineers, concerning the nature and origin of peat and its practical utilization for agricultural and industrial purposes. This brought me in contact with coal and oil groups interested in the chemical composition and origin of these natural formations, especially the role of microorganisms.

My work on composts interested the mushroom growers, who were looking for new sources of organic matter to take the place of horse manure as a nutrient. Because of the rapid replacement of the horse by the motor-drawn engine, mushroom groups began to look around for suitable substitutes. Several large organizations established fellowships in my laboratory for the study of problems of mushroom nutrition. The result was that we were able to throw considerable light upon the general problem of composting and to suggest certain practical solutions for the production of suitable substrates for the growth of the mushrooms.

The results of the studies on the origin and nature of humus can hardly be summarized in a few lines. No flash of genius was involved. The results came about through long hours of detailed investigations of the degradation by microbes of the numerous chemical constituents of plant and animal waste products. Some microbes attack one type of substance, others another. Some of the constituents are easily destroyed, some are resistant and tend to accumulate. Simultaneous with the breakdown processes, microbes synthesize considerable cell material. This together with the resistant plant and animal constituents make up the bulk of the humus. But this too undergoes continuous decomposition by microbes, at a slower rate, however, than the fresh plant and animal wastes. Gradually, the decomposing matter darkens

in color, becomes amorphous in nature, and loosens its original structure.

Humus is thus a newly formed mass of organic matter which is a result of the activities of microbes. It serves as an excellent substrate for the growth of numerous microbes. It undergoes constant decomposition, with the liberation of a continuous stream of carbon dioxide gas and nutrient chemical elements which are essential for plant nutrition. In the soil, the humus is thoroughly mixed with the inorganic residues resulting from rock weathering, thus producing a fertile soil favorable for plant growth. The importance of humus has become so well recognized that it is almost synonymous with soil productivity.

Although many of my students and associates and I devoted years to unraveling the complex microbial and chemical problems involved in the study of humus, we spent some of our time in a variety of related problems. Some of them had an industrial bearing, such as the study of acid fermentations by fungi, others had a public-health value, such as the fate of pathogenic microbes in the soil, and still others opened new horizons for a better understanding of the role of microbes in natural processes.

Among the last, mention may be made here of the study of marine bacteria, carried out at the Woods Hole Oceanographic Institution. When this institution was first organized in 1930, Dr. H. Bigelow, the director, questioned the importance of bacteria of the sea. He could not decide whether these organisms were significant enough to justify the establishment of a special division of marine bacteriology. At the suggestion of Dr. E. G. Conklin of Princeton University, one of the trustees, I was invited to submit a plan for the organizing of such a project. The reason I was asked for an opinion had to do with earlier studies in my laboratory on the bacteriology of the waters and the bottom material of the Bahamas, which involved the problem of lime precipitation. I agreed to spend a month at Woods Hole in 1931. This enabled me to formulate a plan of study of marine bacteria. I became enthusiastic about this new field of research, as another approach to complex microbial populations inhabiting natural substrates. After my plan was considered by the trustees, I was invited to bring it into being. As a result, I spent one to two months each summer during the next twelve years at the Oceanographic Institution. There, assisted by sev-

eral collaborators and students, I searched the sea for bacteria and other microorganisms. I studied their habitats, their activities, and their effect upon the growth of other forms of marine life. This involved occasional trips on the scientific boat *Atlantis*. These trips usually proved to be most disconcerting, for I would become seasick and spend the major part of the time in my bunk. I had to be satisfied with the work of an assistant who usually accompanied me. It gave me a chance, however, to observe the sea at close quarters and to appreciate the significance of some of the problems that I was attempting to solve.

Some of the investigations undertaken could not be completed during the brief summer months and had to be continued at our laboratories in New Brunswick. For this purpose, an assistant was assigned to me by the institution. This arrangement was not fully satisfactory, especially since my main laboratory was, after all, on an agricultural campus. Nevertheless, it led to a considerable body of information on the bacteriology of the sea. After the outbreak of World War II, I gave up this work gradually. Finally, I resigned from active participation and turned over the whole project to a younger man, well trained in this field. The last important project I was concerned with at the institution dealt with the role of bacteria in the fouling of ship bottoms. Since this took place at the outbreak of the war and had an important bearing upon the speed of vessels, it resulted in my close collaboration with the U.S. Navy. I soon withdrew from this project as well, since it required far more time than I could give to it. My last marine experiments dealt with the disappearance of certain groups of bacteria introduced into sea water. This involved the general problem of antibiosis, on which I had now centered all my attention. Upon my resignation from the institution in 1945, I was honored by election to the Board of Trustees.

During my summers at Woods Hole, I had an excellent opportunity to meet many prominent biologists, some of whom became my close friends. I was able to familiarize myself with new approaches to biological problems, some of which had an important bearing upon my own field of microbiology. Here I met protozoologists, geneticists, general biologists, many of whom made outstanding contributions to biology and biochemistry. The institution itself served as host to many outstanding European investigators, notably physiologists and biochem-

ists. They helped me to appreciate better the role of bacteria, not only in the cycle of life in the sea, but also in nature itself. I devoted considerable time, in collaboration with my associates and assistants, to the study of nitrogen transformation, organic-matter decomposition, humus formation, all problems with which I was vitally concerned at that time in the elucidation of the role of microorganisms in the soil. My outlook on microbiology as a whole, and on the role of microorganisms in the cycle of life, was greatly broadened. Thus my time at Woods Hole was not wasted. Aside from offering better living conditions during the hot summer months, it contributed materially toward a better appreciation of biology as a whole and of microbiology in particular.

Until I went to Woods Hole, I had been a landlubber. I had never seen the sea in a true sense, except from the shore or from the deck of an ocean-going vessel. Now I came in very close contact with it. Woods Hole proved also to be a godsend to my family. Bobili would spend hours in or near the water, while Byron became elated over the prospects of studying marine life at first hand. He began collecting shells and ended by taking courses in biology at the Marine Biological Laboratory. His present broad interest in biology is no doubt due, partly at least, to our summer sojourns in that scientific colony.

Although during this period my laboratory at New Brunswick was still a part of a larger department, we were gradually evolving into an independent division of soil microbiology with our own budget, students, and courses. Dr. Jacob Lipman himself, who was officially the head of this department, devoted less and less attention to it. But he was continually helpful in obtaining necessary funds for equipment and supplies, fellowships and assistantships. Although he still had hopes, very vague to be sure, of returning to scientific research upon his retirement, the constantly increasing tasks of administration and the gradual drifting away from scientific interests suggested little probability that this would ever occur. Having long before this turned over to me the management of the work in microbiology, he was always ready with counsel and help.

The latter part of this period was considerably darkened by the heavy clouds that threatened Europe, brought about by the rise of Nazi power in Germany. When many of the scientists in that coun-

try, some of whom were my friends and the objects of great admiration, notably Hans Pringsheim, Carl Neuberg, Otto Meyerhof, were removed from their positions because of their racial origins, when the pernicious doctrines of the Nazis took root in Germany and later spread into other countries—these signs could point to only one outcome, a great world catastrophe. I had ample opportunity to observe the effects in Germany, during our brief visits in 1933 and in 1935, and in other countries, especially in my adopted homeland.

What could I do to fight this destructive effect upon the freedom of people, freedom that had been gained at such high cost, beginning with the English, American, and French revolutions? My own protest could be at most only very insignificant, but it had to be made. First of all, I decided to withdraw my name from two German scientific journals on which I served as a member of the editorial board, the *Cellulosechemie* and the *Archiv für Mikrobiologie*. But this was not so simple as it sounds. Complications developed in each case. The first of these journals was edited by H. Pringsheim and the second by A. Rippel, both of whom had made substantial contributions to soil microbiology.

When Pringsheim was forcibly removed, in 1933, from the editorship of the journal that he himself had established, I wrote the new editor requesting that my name be removed from the board at once. In reply, I received a very polite letter asking that I withdraw this request, since my action would hurt various other members of the staff who were still serving the journal in one capacity or another. I certainly did not want to hurt anybody and decided to withdraw my resignation for the time being. When conditions in Germany failed to improve, however, I again submitted my resignation, which this time was accepted. By a peculiar combination of circumstances, only one issue of the journal appeared without my name; then the journal expired permanently.

In the case of the *Archiv*, my resignation was somewhat more painful. Out of profound respect for Dr. Rippel, whom I knew well personally, I delayed requesting removal of my name as long as possible. When in 1935, however, wholesale attacks upon innocent people and indiscriminate expulsions from universities began in Germany, I felt so depressed that I could not see each new issue of this journal

with my name on the title page. So I wrote a letter to the editor, which I cite in part:

It is with profound personal regret that I have to submit the following request to you. Since you began publishing your esteemed journal, *Archiv für Mikrobiologie*, in 1930, I considered it as a great honor bestowed upon me to have the privilege of being associated with it and to have my name listed as one of its collaborators. Because of my great respect for German science as a whole and because of my profound admiration of the scientific workers in Germany, I felt that I was thus signally honored. During the last years, I was several times on the point of writing to you, expressing my indignation and protest against mistreatment of men of science in your country for the only reason that they belonged to different races or religions, or that they had different political opinions. I felt that this was not only unworthy of a great people but that it bespoke some fault of its vaunted civilization. The main reason for my not doing so earlier was because I was hoping that a change might yet come and save the reputation of a great culture. The savage barbarism that broke out again in your country during the last few days, encouraged and supervised by your government, leads me to but one conclusion, namely, that under these conditions science itself has become degraded. I find no words to give expression to my feelings of shame and disgust at having my name appear on the front page of a journal now published in a country which does not recognize the simplest of human rights. In spite of my profound respect for you and the other editors of the journal, there is nothing left for me to do but to request that you remove my name from the list of collaborators.

I submitted copies of the letter to all the members of the Board of Editors so that they would understand the reasons for my action. I had replies from all foreign members, either approving my action or stating that they understood my position but disapproved because of possible effects upon Rippel's position. One reply, from a member of the board who was residing at that time in this country, was in a different tone:

I hold the viewpoint that in times like the present, it is most important to keep a cool head and to do nothing rash which might prevent people who are evidently on the wrong way from returning to

better reasoning. I have never believed in retaliation as a wise measure, personally or politically. The present situation would not exist at all if the Allies at Versailles had thought of reconciliation instead of retaliation. You will probably claim that the present situation is quite different, but I believe that it is exactly the same. The Allies fought against the imperial government, and then took their revenge, not on the government, but on the people. In the present war between the Nazi government and the Jews, you are in the firing line, you belong to one of the warring parties, and you are not contemplating reconciliation. You feel like the Allies before the armistice. I wish sincerely that you may not feel later as the Allies feel now.

My reply to him was, of course, quite clear and definite:

I heartily disagree with your point of view in your attitude toward the Nazi government in Germany. I would be the last one on earth to defend the Versailles treaty, since I was one who signed protests against it at that time. Further, how can the two situations be compared? One was a political one, no matter whether it was dictated by a spirit of conquest or revenge, whereas the other is purely a humanitarian one dictated by reason of protest against such an attitude of the government of a great people. I believe that the Nazis in Germany represent far more than a mere mob, unless you want to include in such a mob deans of universities who supervise the burning of books, and students, the future carriers of culture, who request professors to leave the university simply because one of their grandfathers happened to have dark hair instead of blond hair. I feel that this bestial persecution of an innocent minority is not a Jewish problem at all but, aside from its purely humanitarian aspects, it is primarily a German problem, which should concern the Germans primarily, especially the liberal and cultural elements, both those forced to live under the present government and those that reside abroad. They can do a great deal for their homeland by raising their voice in protest against this new form of man's inhumanity to man. You know very well that sooner or later the German people will pay for these outrages, if history means anything at all, not through Jewish efforts (who are the Jews, after all, but victims of circumstances always placed in a very convenient position for the governments of the type of the Russian Czars and the German Nazis to utilize in combating dissatisfaction at home?), but through historical forces. At no time in the past have such outrages been committed and remained for very long uncompensated.

Rippel himself felt very bitter about my action, which he took as a personal affront. How many of these honest Germans were like ostriches hiding their heads in the sand and not wanting to recognize the situation for what it was! They had to pay dearly afterward for their cowardice in not taking a definite stand against the vicious and inhumane activities of their government. There were not many German scientists shot or imprisoned for raising a voice of protest.

This was not the end of my battle with the Nazis, however. During the Third International Congress of Soil Science held in England in 1935, the German delegation extended an invitation to hold the Fourth Congress in Berlin, in 1940. Everybody on the Executive Board seemed quite willing, if not happy, to accept this invitation without visible protest. There were two exceptions, however, Dr. Lipman and I. Dr. Lipman stood up and mildly condemned the German government and said that the invitation of a government like that should not be even considered by the society, that he certainly would not attend a congress held in that country, but that if the majority of the board was in favor, he would merely abstain from voting. My own attitude was far more outspoken. Not only did I condemn in no uncertain terms the Nazi government and all those who served it, but also those who were willing to accept favors from it, as an invitation of this nature was bound to be, and would thus be serving as tools for Nazi propaganda. To hold a congress in Germany under these conditions would be to reflect upon science itself and cheapen it on the market place. I said that when the invitation was voted upon, my vote would be negative. Sir John Russell, president of the Oxford Congress, took me aside and argued with me that scientists must not be punished for the action of their government, that there was no other invitation received by the society other than the German, and that a new president must be elected and he had to come from a country where the congress was to be held. My suggestion was to consider Prof. Wiegner as president and Switzerland as that country. There was no formal invitation from Switzerland, however. The final vote was what one might have expected: Dr. Lipman abstained from voting, and I was the only one to vote in the negative. I felt that, under the circumstances, the best I could do was to retire from the presidency of the Third Commission, which I had held since the Washington Congress in 1927. There were

some other considerations for my resignation. I felt that I had done my share in organizing programs of this commission for the first three congresses, and that somebody else should take it over. At my suggestion, Dr. Thornton of the Rothamsted Station in England was elected president of this commission.

The Soil Congress was never held in Germany, because of the outbreak of the war. The last meeting of the Commission on Soil Microbiology was held in my home town at the end of August, 1939, in connection with the International Congress of Microbiology, held in New York on September 1. Unfortunately, two deep shadows were cast upon this meeting: the death of Dr. Lipman, who had helped considerably in making the meeting possible, and the announcement of the German march into Poland, marking the beginning of a new world war. Although a few Germans came to the meeting of our commission, most of them failed to appear at the congress in New York. It was tragic that even the few who came to the New Brunswick meeting did not believe, or did not want to believe, or merely said they did not believe, that the war was coming and that Germany had been preparing intensely for it since Hitler's rise to power, until the German hordes actually began to destroy, kill, and loot the Polish republic.

The meetings of the Soil Microbiological Commission held in New Brunswick were highly successful. There were representatives from numerous countries in Europe and in Asia. A number of important papers were read during the two-day session. At the formal banquet, Dr. Thomas Rivers, president of the Microbiological Congress, which was soon to be held in New York, was the principal speaker. Many old and new friends were gathered to give expression to the science of soil microbiology, the results being published in two handsome volumes. These meetings were followed by an excursion to central and southern New Jersey to examine soil types and agricultural experiments.

The outbreak of the war brought into question the desirability of holding the main Microbiological Congress. Many European groups who had already arrived returned immediately to their homelands; others decided to stay. There was a very heavy registration and the success of the congress exceeded all expectations, both in the number of persons who attended and the high quality of the papers presented.

I took an active part in its organization both as a member of its Executive Committee and as chairman of the Section of Agricultural and Industrial Microbiology. I also managed to attend several meetings of some of the other sections and listened to addresses by masters in the field of microbiology. Because of the great number of outstanding investigators who attended, the congress proved to be a marked success.

Following the congress, we settled down to a hard task ahead of us, the oncoming war. Although our country was not to enter the war for another two years, we knew well enough that it was coming sooner or later. We were all absolutely convinced, however, that no matter how great our efforts would have to be and what price we would have to pay, we would finally win, and freedom would rise again in this world.

Our personal life during this period continued at a rather accelerated pace. Byron grew, and after his preparatory school years, he entered Swarthmore College, where he was to be one of the honor students. During our last prewar journey to Europe, in 1938, he bicycled through several countries. He thus had an excellent opportunity to exercise his knowledge of European languages and to learn to know various peoples, their life and their manners. Bobili continued with her sporadic attempts to study singing and participate in various choral societies. We paid our weekly visits to New York to meet friends and go to theaters and concerts.

Toward the end of the period, I was elected to corresponding membership in the French Academy of Sciences. This was followed, a year or two later, by election to the National Academy of Sciences. The scientific world thus recognized, through me, a field of science that only two decades before was scarcely regarded as a science. I could not help but recall my discussions, in 1918, with Charles Lipman, who argued that workers connected with agricultural institutions, notably experiment stations, could hardly be considered as true scientists, an argument he substantiated by pointing out that there were no representatives from such institutions in the National Academy. Fortunately, both he and I lived long enough to see a whole section on agriculture established by the academy. My own election was in the section of botany, since there was no section on microbiology. I was even hoping that before long this would also be corrected, and the academy would recognize the new science of microbiology.

The fact that microbiology and soil science are borderline sciences, and that workers in these fields have, in the past, received little recognition from scientific bodies, was no better illustrated than in the question of starring American men of science in the biographical directory. In selecting outstanding scientists for starring purposes, the editor considered only twelve fields of science, as recognized at the beginning of the century. In the intervening years new sciences came into being, but he steadfastly refused to give them any consideration. Microbiology and soil science were among these new sciences. My name was submitted for several editions of the directory, but I never received any special recognition, since I would have to be elected under botany, or zoology, or chemistry. Repeated protests to the editor brought forth only the comment that a redistribution of the sciences with each new edition of the directory would be impractical. That he was at least partly right is brought out by the fact that when the new editor later made an attempt to reorganize the various new branches of science, he had to abandon the problem of starring altogether, as a most difficult, if not impossible, task.

I thus completed the fifth decade of my life with an established reputation in my selected science. I felt that the honors I received from various national and international bodies not only fully established my own position in the field of science, but marked my success in helping to bring about the recognition of soil microbiology as an important field of science, so that nobody hereafter would dare to refer to it sneeringly as "ammonification, nitrification, and that sort of thing," or as "a science without scientists, without foundations, and without applications." Little did I dream at that time that the results would far exceed my expectations. The decade that was to follow was to witness the crowning glory of soil microbiology, which was to yield results far exceeding my own wildest dreams. My studies of soil microbes, of mixed populations, of the effect of one microbe upon another, and especially of the actinomycetes—all were to contribute to the opening of a broad perspective, a perspective of which I was to take full advantage, namely, the use of soil microbes for combating disease-producing microbes. These applications were to result in a new field of science, that of antibiotics.

The Second World Catastrophe

TO SAY that we were bewildered by the outbreak of World War II and the events that followed is to put it very mildly. We were stupefied. We did not know what was to come next. All we could do was wait. We were certain of only one thing, that we had to stay at our jobs, work harder, and apply our best to the tasks we were called for. If we had to fight, we would fight. We must win this war at no matter what cost. The destruction of Poland, the gradual deterioration of France, the march of the German hordes into Russia, the ruthless killing off of the Jews and the destruction of Jewish communities wherever the German boot touched the ground, the treacherous attack of the Japanese upon Pearl Harbor, all came with sickening rapidity. All one could do was to grit one's teeth, clench one's fists until they hurt, and carry on. We must not let our civilization be trampled out by the marching hordes. Never since the days of Ghengis Khan was so much destruction brought upon the peaceful populations of the world.

Still, to say that it came unexpectedly is hardly correct. During our previous three visits to Europe we could see everywhere signs of the coming catastrophe: in passing through Germany, in 1933, on our way to Vienna from Copenhagen, where I attended the meetings of the International Soil Chemistry Commission; in 1935, on our way to Moscow from Amsterdam, where I attended the Botanical Congress; and particularly in 1938 in Helsingfors, where I again attended an international soil chemistry meeting. At this last meeting there were a group of German scientists. Their attitude was condescending or

menacing. They were always together, talking to one another in whispers. They reminded me of a pack of jackals waiting for their prey. There was a certain hostile attitude whenever I came in close contact with them, which I usually tried to avoid. People whom I had known for many years, with whom I had had continuous communication since I first began, in 1924, to go to these international conferences, suddenly became antagonistic. There were two or three exceptions, like the plant physiologist Mitscherlich or the potash man Krische, but their attitude was furtive and constantly apologetic.

You could not help but feel that a great storm was in the offing, that these changed relations were manifestations of a coming catastrophe. As we sat in Berlin, in 1935, at a café on Unter den Linden waiting for train connections, and watched the goose-stepping troops appearing from unexpected corners, as we listened to the bloodthirsty songs, we could almost pick out of the air the warnings of dangerous things to come. We became deeply depressed. How the world had changed! Only twenty-five years before, upon our arrival in Germany from Russia we had sung a cheerful, hopeful song: "We have broken the chains from our feet"; now, in 1935, when we left Germany and entered Russian territory, we felt a breath of fresh air taking the place of the thick, menacing atmosphere we had just left behind.

As we were leaving Paris in 1938, our friends the Demolons came to say good-by. Sitting at a sidewalk café and sipping an *apéritif*, Dr. Demolon said to me: "What is America going to do when the Germans attack us again, and, as you must have seen signs aplenty that they are going to do that very soon, what are you going to do to help us?" My reply was: "If you will only hold out for a year, we will come to your help, but not before; we need that much time to prepare." He then rejoined sadly: "Then, we are lost. We cannot hold out a year, even with British help." Those were prophetic words.

The storm came with sudden fury. At the Microbiological Congress in New York we discussed problems that might arise out of the war. Had not the first world war brought forth many new diseases (trench fevers) and epidemics (influenza), for which there were no cures and only little preparation? What new infections and epidemics would this war bring? Might not the new field of antibiotics, with which I had just begun to occupy myself, yield new weapons against

such diseases? I plunged into it with all possible speed, with all the accumulated energy derived from previous decades of work with soil microbes which I was to utilize in search for disease-combating agents.

It soon became evident, however, that I could serve in various other capacities. Before long, I was called upon by various government and other agencies to help in the war effort, in my capacity of microbiologist. The following three or four illustrations will suffice.

The project on fouling of ships' bottoms, mentioned previously, was started in New Brunswick, on a very modest scale. It soon became evident that marine problems could not be studied at an agricultural station, so I moved this project to Woods Hole, where I still had my connection with the Oceanographic Institution. When the war broke out, this project grew to immense proportions, and it was finally taken over bodily by that institution. A number of chemists and biologists were put to work on it. The original question placed before me was: "What role do bacteria play in the attachment of fouling organisms to the bottoms of ships, which result in a reduction in the speed of the vessels?" If such an answer could be found, much time could be saved in cleaning the vessels. Before we could give the final answer to such a complicated question, we found ourselves directing most of our energies toward the practical evaluation of marine paints and their effect on the process of fouling. Having done my share on this problem and having gradually retired from active participation in the actual experimental work, I was asked by the Navy to undertake another important task, which consisted in unraveling the phenomenon of steel corrosion in ships. Since I was already occupied with a number of other investigations and since my associate, Dr. Starkey, had been concerned for some time with the study of the phenomena of sulfate reduction and sulfur oxidation by bacteria and the role of these processes in steel corrosion, he took over this problem. He handled it largely himself with the help of one or two assistants. These studies resulted in an important contribution toward the elucidation of the phenomenon of steel corrosion in the soil and in the sea and methods of control.

I was soon called upon to assist in another serious problem, much nearer to my heart and my field of interest. Immediately following Pearl Harbor it became evident that a major war would have to be

fought under tropical conditions. When the reports began to come in that in the tropics molds or fungi were responsible for considerable damage to optical and electrical equipment, as well as to numerous other service materials, the National Defense Research Council decided upon a comprehensive program of "tropicalization." This consisted in the treatment, against deterioration by fungi, bacteria, and other destructive forms of life, of all service materials that were to go into tropical areas. Here my knowledge of the soil fungi, their activities in general, and their ability to decompose cellulosic and protein materials proved of invaluable help. I was called upon to assist in the development of a program of protection of service materials against such deterioration. At first, my services involved attendance of weekly meetings of joint military and civilian committees in Washington. Later, I had to make various visits to government and industrial laboratories in order to study methods of testing various antifungal agents to determine their value.

Before long, this project developed numerous ramifications. It branched out into every type of military service and involved the protection of every sort of material, ranging from electric batteries and optical equipment to clothing, shoes, and tents. The old methods of evaluating the efficacy of different antiseptic and fungicidal substances had to be abandoned; new methods had to be developed and applied. This involved contacts with various industrial organizations manufacturing different chemicals used as protective agents, also with numerous military establishments, for purposes of education, evaluation, and application of the information obtained. It meant establishment of an information center for the classification and co-ordination of all the available information, the writing of monographs outlining methods of testing and evaluation, and various forms of personal consultations and discussions. Gradually, these problems became so important that special establishments were organized by different branches of the services, so that our central organization, having played its part, came to limit itself to the information center.

Among my other wartime activities, it is sufficient to mention my chairmanship of the War Committee on Bacteriology, under the auspices of the Society of American Bacteriologists. I was elected president of that society in 1941. Although no annual meeting was held

in 1942 because of the war emergency and difficulty of travel, several important problems arose which required immediate attention.

The following year, the new president asked me to organize the War Committee. Since all the government agencies and industrial organizations were now well provided with bacteriological personnel to meet all possible emergencies, there were not many problems that required special consideration by the society as a whole. One of these problems, much talked about at that time, but which later proved to be of little importance, was the reduction in the supply of agar for bacteriological work. Since all the agar used to come from Japan, its supply was cut by the war. The emergency led to a thorough investigation of the domestic supplies of seaweeds, which could be used as sources of agar. Particular attention was given to two common algae growing along the Atlantic Coast which were already being utilized industrially, one (Irish moss) for the production of thickening agents for chocolate milk and other beverages, and another (sea cabbage) for the preparation of chemical agents used in the removal of scales in boilers. I tested the extracts obtained from these seaweeds, but they did not give a firm enough gel to be useful for bacteriological purposes. It developed, however, during these studies that certain materials that could be removed from the Irish moss, following the extraction of the gelifiable extract, did not gel until certain potash salts were added. Although even such a gel was still not firm enough to become a substitute for agar for the preparation of bacteriological culture media, the yield of the gelifying product obtained was increased greatly. Since this material was used as a solidifying principle in a variety of food products, such as ice-cream making, and in certain industrial processes, such as paper sizes, it was of some economic importance.

Thus, my time was greatly occupied by a variety of theoretical and practical problems resulting from the war. I was called upon to assist in their solution because of my knowledge of microbes, their occurrence in the soil and in the sea, their life activities, and their role in natural processes. This experience paid off in another quite unexpected way.

I have already mentioned that I became interested, just on the eve of the second World War, in a new field of research, that of antibiotics.

This proved to be the culminating point of my microbiological career. In order to understand why one who has devoted his whole life to the study of soil microbes should suddenly become interested in problems concerned with combating infectious diseases, certain explanations are in order.

Just what are antibiotics? They are products of microbial growth. They have the peculiar property of inhibiting the growth or even of destroying other microbes. More important, they are active not only in the test tube, but also in the human and animal body. They also have a selective action upon different bacteria and other microbes, and may thus be destructive to some organisms and not to others, be injurious to pathogenic organisms and not to cell tissues. They are thus quite different from ordinary antiseptics and disinfecting agents, like phenol and mercury. Although efforts had been made to utilize antibiotics for combating infectious diseases prior to 1939, it was on the eve of the second World War and during the first years of this war that this phase of microbiology, which was to revolutionize medical science, took a new lease on life. I shall discuss these developments, and my own part in them, in detail later.

The study of antibiotics produced a profound change in my work, in my relations to the outside world, and in my scientific contacts. My former interests had been devoted to the solution of soil problems, to the effect of the soil upon the growth of crops, to the role of microbes in soil and in marine processes, to the utilization of microbes in connection with certain industrial developments. In all my previous experience with microbes, I barely thought of problems concerned with causation of disease. It is true that I had made some study, at various times, of disease-producing bacteria, their reactions in the human and animal body, their survival in the soil and in water basins. I had had some experience, during 1918-1920, when I worked for Takamine, with the first true chemotherapeutic agent, salvarsan. But in spite of all this, I was primarily a soil microbiologist, studying soils and composts, peat bogs, and manure piles. I was primarily concerned with products of microbes that are used by green plants, be they diatoms in the sea or grasses and trees growing on land, or with products of microbes, such as enzymes, organic and inorganic acids, and alcohols, used in a variety of industries. Prior to 1939, I scarcely dreamed of be-

coming profoundly involved in problems dealing with human and animal diseases.

I must emphasize further that among the various microbes that aroused my particular attention were the actinomycetes. Was not my very first scientific paper, covering the work that I did in collaboration with Curtis in 1915, devoted to this group of microbes? Was not the first address that I prepared for a scientific society in 1915 devoted to the occurrence of actinomycetes, fungi, and bacteria in soil? Thirty-five years later, in 1950, it was the actinomycetes that were yielding the most important antibiotics. Since the rediscovery, in 1940, of penicillin, a mold product, it was largely the antibiotics of actinomycetes that were used to combat infectious diseases ranging from typhus fever to tuberculosis, from typhoid to tularemia, from dysentery to plague.

I shall discuss in detail later the various unexpected fields of human endeavor with which my work on antibiotics has brought me in contact. Meanwhile, these were the war years. There was still the grinding work from early morning to late at night. Fortunately, the results were highly rewarding. A brief enumeration of them may be justified here.

The new antibiotics isolated in my laboratory came in rapid succession. One might say that with every spade of soil, new diamonds were uncovered. Every new chemical compound that we discovered in the metabolic fluids of microbial cultures was a step forward in the solution of the antibiotic problem, the combating of infectious diseases. First came actinomycin in 1940; it proved to be very toxic. Although it showed certain interesting chemical and pharmacological properties, we put it aside because of lack of promise as an antibacterial agent. Years later this antibiotic was found to be effective against Hodgkin's disease. Clavacin, which came in 1941, was less toxic, but not yet what was wanted. Another substance isolated about that time, fumigacin, was also less toxic but markedly less active. Streptothricin was isolated in 1942. This antibiotic almost gave us the answer to the problem, since it was active against the bacteria not touched by penicillin. Unfortunately, it also possessed a certain delayed toxicity. It was now a matter of continuing our search to find a substance with properties similar to streptothricin but less toxic.

Streptomycin, discovered in 1943, met these specifications. It was

similar to streptothricin, but less injurious to the animal body. Had we not gone through these various steps of isolation of one antibiotic after another, beginning with actinomycin and ending with streptomycin, we would undoubtedly have failed to recognize the latter for what it was. This is borne out by the fact that although the organism *Streptomyces griseus* was first isolated by Curtis and myself in 1915, we did not test it for antibacterial properties, which it may have possessed, as we eventually demonstrated. At that time, however, we were not interested in antibiotics. Twenty-eight years later, we discovered streptomycin, because we were looking for it, because we had studied previously this type of compound, because streptothricin that preceded it had similar chemical and biological properties, and because we had already worked out detailed procedures for the production and isolation of this type of compound.

These findings did not come to us by mere luck or by chance discovery. They were the result of much painstaking work, after much planning and preparation. The program soon developed on such an extensive scale that I required more and more help, both in my own laboratory and from outside sources. More and more students and investigators came to the laboratory to assist me in this task and become familiar with a new field of science and application. In the antibiotics program I was assisted by nearly fifty graduate students and visiting investigators. Without them I could never have accomplished what I did. They were the fingers of my hand, which, as a unit, accomplished a great task. This teamwork might be compared to that of an orchestra, with the conductor leading and assigning the task to each member, none of which would have produced any symphony otherwise.

In the solution of this problem, I received considerable help from certain philanthropic organizations, notably the Commonwealth Fund and the Lasker Foundation, and certain industrial concerns, especially Merck & Co.; this was followed later by assistance from various clinical organizations, notably the Mayo Clinic. In 1938 Merck established a fellowship in my laboratory for the study of industrial fermentations. This was supplemented in 1939 by a grant for the study of antibiotics. We drew up an agreement with that company, whereby they were to help us in our work by providing chemical assistance,

experimental animals for pharmacological evaluation of new antibiotics, and large-scale equipment for the production of promising antibiotics. In return, we undertook to assign to the company any patentable processes that might result from our studies. It was agreed thereby that should such patents prove useful, the company was to pay to the university a small royalty (two and a half per cent).

This agreement proved to be of the greatest importance in my work. Without it, most, if not all, of the antibiotics that we isolated would have remained bibliographic curiosities. Soon after the isolation of streptomycin, however, when the potential importance of this antibiotic in the treatment of numerous human diseases was established, it became evident to me that both the university, which was now a state institution and of which the Experiment Station was an integral part, as well as I personally might be subjected to criticism for having turned over valuable public-health processes to a single organization for commercial exploitation. The director of the Experiment Station and I, therefore, presented to Merck a plan whereby all other interested organizations would be able to manufacture streptomycin under an arrangement similar to that made with this original company. The response was magnanimous; the company accepted all our recommendations, thus saving me personally and the university in particular considerable embarrassment and possible ill will on the part of the public as a whole for trying to establish absolute control of a process that was to save human lives.

The university then proceeded to set up a Research Foundation to handle all matters pertaining to patents and license arrangements. All the patents that had meanwhile been applied for by Merck and that covered the various antibiotics were reassigned to this foundation. I had experienced considerable difficulty before this arrangement went into effect. Long before the discovery of streptomycin, when we first recognized, in 1941–1942, that streptothricin might become an important chemotherapeutic agent, we received numerous requests for cultures and for information about the production of this antibiotic. These requests came with increasing tempo when streptomycin appeared on the horizon. I complied with all the requests in such a manner as not to discourage scientific investigations but not to allow irresponsible organizations to misuse our discoveries for unwarranted

applications. When the requests came from research workers in universities, the cultures were sent out at once, with the sole request that they not be given out without my permission. When the requests came from industrial organizations, these were asked to sign a gentleman's agreement to the effect that if they proceeded with the manufacture of the antibiotics discovered in our laboratory, they would guarantee the payment of a small royalty to the university in a manner comparable to that of other organizations. When the Merck people finally agreed to release us from the single-license clause, we were fully prepared to take advantage of the wide potential distribution of streptomycin. Subsequent events justified this policy. The cultures and all available information were sent freely to various laboratories throughout the world. These type cultures made possible further careful studies of other antibiotics in university, government, and industrial laboratories. On the other hand, in preventing indiscriminate use of these cultures and of the antibiotics produced by them, we avoided potential frauds.

Numerous mysterious telephone calls began to reach me offering untold sums of money if only I would refrain from publicizing my results and turn them over to private exploitation. My answer was an emphatic "No."

The cultures for streptothricin and streptomycin production were sent out to some one hundred prospective manufacturers in this country and abroad. Numerous laboratories made an effort to produce these antibiotics, but had to give up because of difficulties in the practical exploitation and the rapidly growing competition. Finally about eight important companies remained in the field. They represented the broad chemical and pharmaceutical fields in the United States. They proceeded at once with the development of suitable processes of manufacture of streptomycin. They all remained faithful to their original arrangements, as will be told later. Many foreign organizations followed. This gave rise to new problems.

So much for my scientific pursuits and their practical developments during the war years. My family affairs continued at first in a rather normal fashion. Byron was interested in research, especially in problems connected with certain systemic diseases of the type of rheumatic fevers. We decided that he should enter a medical school.

The idea behind this decision was that a medical degree would facilitate his entrance into hospitals, a primary requisite for the type of work he would like to carry out. He made several applications to medical schools and was accepted by the University of Pennsylvania. Here, after three and a half years, the last year being somewhat shortened by the war emergency, he received his M.D. degree in 1944. This was followed by an internship lasting only nine months, at the Michael Reese Hospital in Chicago. While there, he fell in love with and married a cadet nurse, born in Michigan, largely of French stock. Our small family thus received a new member, Joyce. Unfortunately, our joy was soon marred by Byron's departure for the battle front. He joined the Army on October 1, 1944, with the rank of Second Lieutenant, and, within three months, received orders to go abroad as a member of the staff of one of the general hospitals. He was the last to complete a hospital unit, ready for shipment to the European theater of war. His letters were cheerful but made us feel anxious, as when he wrote from the ship: "We are now crossing the Channel and, as I write, I watch the furniture pass by." What he did not tell us was that the Channel was infested with German submarines, that the ship carried a load of explosives, and that depth bombs were being constantly dropped from it.

During the next six months, or the last period of the European war, we were in continuous suspense, daily awaiting news from him, our only child. Fortunately, he came through the war well in body and in spirit. He was kept in the Army for another sixteen months after the war, being stationed with a hospital unit first in Marseilles, then in Bavaria with the Army of Occupation. He finally returned home, a Captain of the Medical Corps. Upon his discharge, he proceeded at once with his interrupted course of training in medical science.

We thus lived through the whole war period in a sort of daze, especially during the months that Byron was abroad. We were either at work or at home glued to the radio for news and more news. Finally, the war was over. It was won. We were all exhausted. What was to come next? For me personally, a new era had begun. I was now in the midst of antibiotics development, especially since the work in my laboratory resulted in the introduction of new techniques, in the recog-

niton of new types of organisms as potential producers of antibiotics, and in the actual isolation of several promising chemotherapeutic agents. Since practical developments in the field of antibiotics were made largely in the United States during the war period, it was, therefore, quite logical that my work should have had focused upon it universal attention. There were a number of contributing factors. Among them was the writing of a series of reviews and books in the field of antagonistic relationships among microorganisms and the production of antibiotics, the first in 1937, the second in 1941, and the third, a comprehensive volume, in 1945. In 1941 I first suggested the use of the word "antibiotic" to designate the group of compounds produced by microorganisms and having a growth-inhibiting effect upon other microbes. Finally, my practical knowledge in the field of microbiology and especially its application to antibiotics, largely gained through my association with Merck and later with other companies, led to further developments.

I was now receiving numerous invitations to deliver lectures and addresses before various universities, state and national scientific societies and academies, professional organizations, clubs, hospitals, and other groups. I thus addressed, early in the period, various medical groups, beginning with the University of St. Louis, numerous chemical societies, beginning with that in Chicago, university clubs, such as those of Florida, many academies, beginning with the National Academy of Sciences in Washington, and others. Soon these invitations began to arrive at the rate of one or more a day from all over the world. I was forced to decline the great majority of them.

In Search of Antibiotics

IT IS rare that in the history of science, a new and unexpected field, with unlimited horizons, opens up before the eyes of the searcher. The steps that led to such an event, in my own case, can best be illustrated by the following story.

In October, 1944, I was invited to deliver a lecture before the members of the staffs of the Mayo Clinic and the Mayo Foundation, in Rochester, Minnesota. At an informal luncheon preceding the lecture and attended by a group of about thirty members of the scientific and clinical staffs, the president of the foundation, Dr. D. C. Balfour, made certain pertinent remarks and raised an important question, which can be transcribed somewhat as follows:

"We have with us today a representative from an agricultural institution, one who has no medical degree and has never even received any training in medicine. He is going to address us tonight on a subject that is at the moment of great importance to medical science and clinical practice, dealing with the discovery and application of new chemotherapeutic agents in the treatment of infectious diseases. The fact that you, Dr. Waksman, have been invited to deliver an address before a great medical organization, such as ours is, suggests that you are bringing to us a very important message. We would like to know, however, through what stages of scientific progress and discovery you have arrived at such a position. Perhaps you might be willing to share with us at this time, in as informal a manner as you can, the story of how your previous training and experience has made it possible for you, by digging in the soil and by searching for perfectly harmless

microbes, to succeed in unraveling some of the mysteries of nature and to uncover important principles and make scientific contributions that promise to be of great benefit to mankind and to further the process of healing of important diseases that did not lend themselves previously to therapy."

I responded to this gracious invitation by summarizing the story of my work since my graduation from college, as presented in the previous pages. I emphasized certain aspects of this work which led me to the study of antibiotics, and which finally resulted in the isolation of streptomycin. I drew particular attention to the beginning of my research career, when, as a senior at Rutgers, I had to undertake the preparation of a thesis which was required for graduation. I said, among other things:

"Since my major field of study in college was soil bacteriology, it was only logical that I should have selected as a subject for my thesis a problem dealing with the bacterial population of the soil. After consultation with my professor, I decided that a survey of the distribution of bacteria in different soil types, at different depths and in different seasons of the year, would yield the desired experimental data. Fortunately, several soil types were found on the grounds of the College Farm, representing clay, loam, and sandy formations. I immediately proceeded to dig several ditches, to a depth of thirty inches, using proper bacteriological techniques. The samples were brought to the laboratory, plated out, and after proper incubation, the colonies developing on the plates were examined and counted. My professor was a leading authority on methods of study of soil bacteria; he always emphasized the importance of using synthetic media for the enumeration of these organisms. These recommendations were followed carefully. Little did I dream at that time that there was gold in those ditches, in the form of microbes, many of which had never been seen before by the human eye.

"In addition to the colonies of fungi and bacteria, I found the plates to contain minute colonies that were neither one nor the other. They were, however, sufficiently similar to those of bacteria to be usually reported as such. When examined under the microscope, however, the growth appeared to resemble that of fungi. My professor spoke of them as colonies of higher, or filamentous and branching bacteria; he con-

fessed frankly that although he had seen them many times before he had not paid much attention to them and knew little about them. I soon learned that they belonged to a rather obscure group commonly known as actinomycetes.

"Month after month, during the fall, winter, and spring of 1914-1915, I proceeded to dig the ditches, to plate the samples of soil, and to record dutifully the numbers of fungi, bacteria, and actinomycetes present in a gram of soil. When the results were finally tabulated, there was another, perhaps even greater, discovery in store for me. I found that not only did the numbers of the three groups of organisms differ in relation to the nature of the soil, but their relative distribution also varied with depth of soil. The numbers of bacteria as well as their relative proportion to the total number of microbes diminished gradually from the surface of the soil downward; whereas in the surface layer the bacteria comprised eighty or more per cent of the total number, the thirty-inch layer contained only sixteen to forty per cent. The number of actinomycetes, however, diminished only slowly with increasing depth, and their proportion gradually increased, from seven to twelve per cent at the surface to fifty-three and even as high as eighty-four per cent at thirty inches.

"I was so fascinated by these results that I decided at once to undertake a further study of the actinomycetes population of the soil. A proper opportunity was presented to me when, upon my graduation from college in 1915, I was offered a research assistantship at the New Jersey Experiment Station. I concentrated upon the fungi and actinomycetes. I soon became convinced that microorganisms in the soil do not live in watertight compartments, separated from one another, but that they comprise a rather complex group of interrelationships. One organism may be able to assist others, by providing energy and nutrients; some organisms are capable of destroying others, either by actually consuming them or by otherwise interfering with their growth.

"I was thus impressed, at the beginning of my scientific career, by two important principles that were to serve as guiding lights in my whole future work, namely, the recognition that the soil is made up of a large number of different groups of microorganisms, each possessing different functions and activities, and that these microorganisms influence one another in a variety of ways. When the year's work

was finished, several scientific papers were ready for publication. They dealt primarily with the fungi and actinomycetes of the soil, their nature, abundance, distribution, and possible role in soil processes. I was now convinced that my preparation for a scientific career in the field of microbiology was not adequate. After all, it was not the microbes themselves and their distribution in nature that had the greatest appeal to my imagination, but what these microbes do, what part they play in natural processes, how they participate in the manifold natural transformations, and how they influence soil fertility and plant growth. To elucidate these activities, further training in chemistry, especially in biochemistry, was absolutely necessary. With this in view, I proceeded to California, where I spent nearly two years working on the biochemical activities of soil fungi and actinomycetes.

"When I returned to Rutgers my first task was to complete my earlier studies on the nature and abundance of the actinomyces population of the soil and their physiological and biochemical activities. In spite of the fact that most of my time was taken up during the next two years by an industrial position and that a great many hours had to be spent each week in developing a course on soil microbiology, which I was offering to a group of graduate students, all the time available for research was devoted to the actinomycetes. In 1919 I completed a monographic treatise covering my studies on this group of organisms; this was followed by several papers on their nutrition and biochemical activities. In these, I attempted to elucidate the relationships between the actinomycetes and the other members of the microbiological population of the soil, and suggest their potential activities in soils and in composts. Other studies, carried out later, on the effect of partial sterilization of soil upon the abundance and activities of microorganisms tended to establish the fact that in a normal soil, microorganisms live in a state of equilibrium; any disturbance of this condition, as can be produced by heating of the soil or by treating it with volatile antiseptics, and even by consecutive drying and wetting, freezing and thawing, results in a change in the relative numbers of the various groups of organisms. The actinomycetes were found to be among those most resistant to such treatments. They were less sensitive than the fungi and bacteria and underwent fewer fluctuations.

"Certain ideas began to crystallize gradually in my mind. These laid

the basis for the future development of the subject of antibiotics. Thus, in 1925 I found that a combination of two organisms leads to the formation, from proteins and amino acids, of products which are not formed by any one organism alone. In 1932, I received a grant from the National Research Council to study 'the fate of acid-fast bacteria in the presence of free-living organisms of the soil, as providing the basis for a chemical study of a well-known group of bacteria which are of great economic importance in relation to one of the most common members of the natural population of the soil.' Following a series of studies on the effect of one organism upon another in the decomposition of plant residues, I completed in 1936 a comprehensive investigation on the associative and antagonistic effects of microorganisms. These results emphasized the fact that actinomycetes are able to exert a considerable influence upon the activities of fungi and bacteria in the soil.

"Thus, toward the beginning of 1939, the various approaches to the subject of the interrelationships among microorganisms in the soil seemed to point to the advisability of my undertaking a detailed study of the effects of various microorganisms, notably the fungi and actinomycetes, upon disease-producing bacteria. At that time the major activities of my department were still concerned with soil microbes and not with disease-producing organisms. A proper stimulus was, therefore, needed before the results of all previous investigations could be synthesized and directed into this channel.

"Two events occurred that year which provided the necessary stimulus. One was the work of a former student on the production by soil bacilli of substances (gramicidin, tyrocidine) which have a destructive effect upon disease-producing bacteria; I felt from my past experience that the fungi and actinomycetes would provide far more effective antibacterial agents than the bacteria. The second stimulus was provided by World War II, which was then looming on the horizon, and which pointed to a need for new agents for the control of various infections and epidemics that would no doubt arise.

"After due consideration of the problem, I decided to direct my own efforts, and those of most of my graduate students and assistants, to the elucidation of the processes involved in the destruction by sapro-

phytic soil microorganisms of bacteria causing human and animal diseases.

"With the background of my work on soil microorganisms, it was only logical that I should turn to the soil in search of agents active against pathogenic bacteria. After various preliminary studies on the pyocyanus organism as a producer of antibiotics, I gradually concentrated on the fungi and actinomycetes. The first antibiotic which we isolated in our laboratory in 1940, from a culture of an actinomyces, was designated as 'actinomycin.' The major importance of this discovery was the fact that it pointed to the possibility of utilizing actinomycetes as sources of antibiotics.

"New methods for testing freshly isolated cultures of microorganisms for their antimicrobial potency were gradually developed or adapted from older procedures. The whole principle of screening was thus elucidated. With each subsequent step in the development of the antibiotics program in my laboratory, new progress was made in the understanding of the type of antibiotic that was wanted, and in methods for its production and isolation. Each new substance brought the final goal of obtaining proper chemotherapeutic agents nearer and nearer. The progress was so rapid that I soon felt that the time had come to make a careful analysis of the literature on the subject of antibiotics and to point to future potentialities.

"In summarizing the antagonistic relations of microorganisms, I wrote in 1941: 'Ranging between the phenomena of true parasitism, where one organism lives in or upon the living body of another, and true saprophytism, where one organism merely destroys the waste products and dead cells of the other, there is a group of relationships between living systems which may be designated as associative and antagonistic; in the first, one organism assists the other, whereas in the second, one organism is injurious to the other. There is increasing appreciation of the fact that nature harbors many unknown organisms that are capable of combating disease-producing bacteria, fungi, worms, and insects. Our knowledge of the activities, potentialities, and importance of these microbes is still incomplete. Man, in his struggle for existence, succeeded, before the development of microbiology, in domesticating and utilizing the activities of many microbes. However,

these represent only a small fraction of the microbial world. It is possible that we are finally approaching a new field of domestication of microorganisms for combating the microbial enemies of man and of his domesticated plants and animals.'

"The isolation of actinomycin was followed by the isolation of other antibiotics, which lacked, however, for one reason or another, the desirable properties which would make them suitable for therapeutic purposes. Finally, a very interesting antibiotic was obtained from a culture of an actinomyces; it was designated as 'streptothricin.' This substance was readily soluble in water, was not soluble in organic solvents, and was stable to heat. It was active against both gram-positive and gram-negative bacteria, *in vitro* and *in vivo*. It possessed only limited toxicity to animals. Here was a new type antibiotic that could easily supplement penicillin where the latter left off. Its importance was immediately recognized by several industrial organizations, who undertook pilot plant study of its production and a survey of its ability to control infectious diseases. Unfortunately, it soon became recognized that this substance as well was not an ideal chemotherapeutic agent, since it tended to exert a delayed toxic effect in the animal body.

"The discovery of streptothricin was, however, a great step forward in the further search for antibiotics in my laboratory. The rapid progress that was being made in 1941-1942 in the production and isolation of penicillin made it evident that an ideal chemotherapeutic agent for the treatment of diseases caused by gram-positive bacteria had already been found. The great need was, therefore, for a substance which would be just as active against the gram-negative bacteria and against the tuberculosis group of organisms, both of which comprise some of the greatest scourges of man and of his domesticated animals. Streptothricin possessed such properties. It appeared now beyond doubt that further search for this type compound would yield preparations possessing similar properties, but less toxic than streptothricin. We proceeded at once with our screening program and with the selection of new cultures producing similar antibiotics. In September, 1943, my assistants and I succeeded in isolating in our laboratory an organism that produced an antibiotic which possessed properties similar to those of streptothricin but that was less toxic. This organism was similar to

one which Curtis and I first isolated in our early work in 1915; it was designated at that time as *Actinomyces griseus*. The new antibiotic was named 'streptomycin' after a new generic name, *Streptomyces*, that Dr. Henrici and I had assigned, only six months before, to the group of actinomycetes which produced sporulating aerial mycelium.

"The progress made immediately in the development of streptomycin was phenomenal. Within four months after the isolation of the streptomycin-producing culture, the active substance was obtained in concentrated form and its antibacterial properties were determined. The isolation of this antibiotic was announced in January, 1944. Soon afterward, its potency against the tuberculosis organism, its limited toxicity to animals, and its effectiveness *in vivo* against various bacteria were established. We are now ready to determine its clinical potentialities."

This was the story I told the medical investigators at the Mayo Clinic. That evening, I delivered before the assembled scientists and medical workers of the Mayo groups an address on streptothricin and streptomycin. It aroused general interest. Within the next few months, there appeared some fifty papers on the antibacterial and pharmacological properties of streptomycin, its activity in experimental animals, and its clinical effectiveness. Several industrial companies became interested in the manufacture of streptomycin, and a few patients were treated in 1944, with some very striking results.

The following year marked phenomenal progress in the production and utilization of streptomycin. Its activity against experimental tuberculosis in animals, first demonstrated by Dr. William H. Feldman and Dr. H. Corwin Hinshaw of the Mayo Clinic, was fully confirmed. Soon afterward, its favorable effect upon clinical tuberculosis was definitely established. On January 19, 1946, Dr. Hinshaw addressed the first large scientific assembly on antibiotics, arranged by the New York Academy of Sciences, on the subject of clinical uses of streptomycin. He dedicated his address to the second anniversary of the publication of the first paper on streptomycin.

The third year (1946) witnessed great progress in the manufacture of streptomycin and its use in the treatment of numerous bacterial infections, notably in the therapy of tuberculosis, the "white plague" of man. A rapid reduction in price made possible wide use of the drug.

Its application now extended beyond the limits of the United States. Under the leadership of the Food and Drug Administration, uniform standards were established for activity, toxicity, and keeping properties. Government control of its distribution was gradually relaxed. Within a very brief period, the manufacture of streptomycin grew into an industry with more than fifty million dollars annual productivity. Its effectiveness in numerous forms of tuberculosis and in many other human infections had now been established beyond doubt.

At the end of 1953, nearly ten thousand scientific papers and some twenty volumes had been published dealing with streptomycin, especially its activity in clinical medicine. Its manufacture in the United States alone increased to nearly twenty million grams per month. In addition to eight American companies, three companies in France, two in England, four in Japan, one in Sweden, two in Italy were manufacturing streptomycin, and several other plants were being erected in other countries. At the end of 1953, the production of streptomycin in the United States and in various foreign countries was still on the increase.

The work on antibiotics in my laboratory did not end with streptomycin. We continued our search for microbial agents more effective than streptomycin and less toxic. Within a period of ten years, we succeeded in isolating a series of antibiotics, largely from actinomycetes. These included grisein, neomycin, streptocin, fradycin, ehrlichin, candidin, candidin, and certain others. The end is not yet in sight. Whether others will be better than streptomycin or whether they will supplement it, the future will tell. It is now definitely established, for example, that neomycin has found a definite place in the treatment of various diseases by topical and oral routes.

In the meantime, numerous industrial and educational organizations began to study actinomycetes for their ability to produce antibiotics and vitamins. It is said that more than one thousand research workers are now engaged on this task. To meet this growing interest, I published in 1950 a book on the actinomycetes and wrote in 1953 a volume entitled *A Guide to the Actinomycetes and Their Antibiotics*.

The isolation in various industrial laboratories of such clinically valuable products as chloramphenicol, the tetracyclines, erythromycin,

carbomycin, viomycin, bears evidence that the results obtained fully justified the expectations that other antibiotics produced by actinomycetes can find an important application in chemotherapy. Actually, among the antibiotic-producing groups of microorganisms, the actinomycetes are at present the most promising. The formation of vitamin B₁₂ and possibly other vitamins points to the possibility of isolating other valuable substances that affect human health. An active search also goes on for agents effective against viruses and neoplasms.

From mere laboratory curiosities, the actinomycetes of the soil have been made to yield highly important chemical substances, which are used for combating numerous infections of man, animals, and plants, as well as for improving the growth of men and animals. They are thus justifying the emphasis that fundamental scientific investigations will sooner or later lead to practical applications. It is essential to gain fundamental knowledge; the application will come sooner or later.

As one looks back to three and a half decades of investigations of the microbiological population of the soil and its role in soil processes and in human welfare; as one watches the growing knowledge of a rather obscure group of microbes, the actinomycetes, one becomes conscious of certain trends in the clarification of our knowledge of natural processes. No better illustration can be selected than the case of these organisms. When I first observed them in 1914, never having seen them before, I was struck by their ability to produce, under certain conditions of culture, highly characteristic odors and pigments; their macroscopic and microscopic appearance and their peculiar type of growth on artificial media were quite different from that of true fungi and bacteria. During the following twenty-five years, I, together with my associates and students, isolated and described numerous new species belonging to this group of organisms. We had succeeded in gaining considerable knowledge concerning their physiological and biochemical activities, their distribution in nature, their taxonomy, their possible role in soil processes and in composts. This was finally followed by a decade of study of their practical utilization for the production of antibiotics. Little did I dream that the time might come when this accumulated knowledge concerning a highly obscure group

of microorganisms would lead to the establishment of powerful industrial organizations for the manufacture of chemical substances used in the eradication of numerous infectious diseases.

In 1941, accompanied by two of my assistants, I visited Merck & Co., the industrial organization mentioned earlier which had collaborated with us in industrial fermentations and in the production of antibiotics. When we isolated actinomycin, I wanted to utilize the large tray facilities of that company for growing microorganisms, to produce a large amount of the antibiotic required for chemical and animal studies. When we were about to inoculate the large chamber with a spore suspension of the actinomycin-producing organism, I said to my assistants: "You are now witnessing a historical event. It is the first attempt that has ever been made to grow an actinomyces on a large scale, to attempt to utilize an actinomyces for any practical purpose, or even to find any use whatsoever for a member of this obscure group of microorganisms." Only a decade later, numerous industrial organizations throughout the world were engaged in growing these organisms, using 20,000- to 60,000-liter fermenters, on a scale never dreamed of before. The various antibiotics produced by them have become bywords. These products have placed in the hands of the clinician powerful tools to combat numerous infections that previously did not lend themselves to therapy.

When on my voyage to Spain in 1950 and to many other countries soon afterward, mothers and fathers brought me their children to bless them, because they owed their lives to streptomycin, and men and women rushed to kiss my hand for having saved their loved ones, I could visualize more clearly than ever before how important the actinomycetes had become in the production of therapeutic substances used in saving human lives.

As I look back to the scientific contributions made to the knowledge of actinomycetes in a rather small laboratory in a small American college, I wonder how much of this is the product of a single individual investigator or the collaborative efforts of many workers. How much has the individual contributed to this development, how much does an individual initiate or guide a series of investigations, and how much does the collaborative efforts of a group or a team result in the solution of a fundamental problem and the evolution of new applica-

tions? There comes to my mind the story of an ancient man of learning, Rabbi Shmuel, who said, when asked how one human being was able in a single lifetime to accumulate as much knowledge as he possessed:

I owe a lot to my teachers; they taught me well, they taught me the facts and ideas that helped me lay the foundations for my future. I owe more to my friends, because they encouraged me to continue my search for knowledge; they supported me in periods of depression; they collaborated with me in interpreting observations and in coming to conclusions; they helped me when help was needed. Most of all, however, I owe to my students; they continuously questioned me; they made me re-examine and re-evaluate my ideas; they stimulated me to search for new information and to obtain new facts in order to satisfy their insatiable curiosity; it is, therefore, to them that is due the greatest debt of gratitude for what little I have attained, and it is they who will continue the search for new knowledge after I have passed away.

In similar terms I may say that I owe a great debt to some of my teachers, notably Dr. J. G. Lipman and Dr. B. H. Halsted. I owe more to my friends and my colleagues, some of whom were formerly my students and later became my friends and collaborators. The greatest debt, however, I owe to my students. I owe much that I have accomplished to those men and women who came to my laboratory from all corners of the earth, to share with me in the search of the unknown, to add to the fund of existing knowledge of actinomycetes and other microbes, to contribute not only actual laboratory assistance, but also many of the original ideas presented in this work. To name only a few would be a disrespect to others; their names have been engraved in the work that has been done in my laboratory. It is they who, in one capacity or another, have helped to build this monument of knowledge to a little-known group of microorganisms that have found such wide practical applications.

The collaboration between Science and Industry can be well illustrated by the association between my department and Merck & Co. When we first isolated actinomycin in 1940, they spent considerable time in elucidating its chemical structure and used nearly ten thousand experimental animals in determining its toxicity and activity. When we isolated clavacin in 1941 and streptothricin in 1942, the same story

was repeated but on an even grander scale. Their bacteriologists and engineers developed special techniques for large-scale production of these antibiotics, their chemists succeeded in obtaining highly purified preparations, their pharmacologists carried out numerous studies in establishing the potential activities of these antibiotics in experimental animals. Each new antibiotic that we isolated proved to be more promising than the previous one. Although streptothricin was found to possess a certain delayed toxicity upon animals, the company was prepared to submit it to clinical experimentation.

In our continued search for substances that would have similar properties to streptothricin and be less toxic, we isolated streptomycin, a preparation that appeared to be the substance that we were looking for. In accordance with the agreement we had made with Merck, we brought this to the immediate attention of the company. A large-scale study of this antibiotic was undertaken at once. As soon as its potentialities against various infectious diseases, especially tuberculosis, were visualized, the company distributed, freely and widely, considerable quantities of the material to various laboratories and hospitals for detailed clinical trials. So extensive were these studies and so interesting the results that in June, 1945, they invited to their plant in Rahway a large group of bacteriologists, clinicians, and pathologists, to go over the experimental data on streptomycin and to offer specific recommendations. The proceedings of this conference were mimeographed in a comprehensive volume.

In accepting the annual award from the American Pharmaceutical Manufacturers Association in June, 1949, I said:

Science today, and this is true particularly of biological sciences, requires the close collaboration of investigators from various fields. The microbiologist has to depend on the biochemist, on the organic chemist, on the pharmacologist, the physiologist, and finally the clinician to help him in the solution of the many problems in which microbes play an essential role. The close collaboration between the microbiologist and industrial organizations interested in the utilization of the manifold potentialities of microbes is an absolute requirement, without which little progress can be made. Microbiology as a science is only now coming into its own. Until now, it has been largely the handmaiden of the pathologist, the agriculturist, and the food technologist. In becoming

an independent discipline, microbiology has much to offer. Only the closest collaboration of the laboratory worker in the university and in the government laboratory with industry will make possible the most effective utilization by man of the potential activities of those millions of microscopic forms of life inhabiting every substrate around us which still await the inquiring mind.

Another form of collaboration proved to be highly important in connection with our work on streptomycin. Soon after our announcement of the isolation of this antibiotic and of its effectiveness upon the tuberculosis organism, Dr. Feldman and Dr. Hinshaw, of the Mayo Clinic, came to see me. They requested permission to test the effectiveness of streptomycin in experimental tuberculosis. Immediate arrangements were made to supply them with small amounts of the antibiotic, so that guinea pig experiments could be started at once. Later, I introduced them to Merck, recommending that they be supplied with larger amounts of the material. The very first animal experiments demonstrated beyond doubt the effectiveness of streptomycin in the control of experimental tuberculosis. In 1945, clinical trials were started; the results fully confirmed the animal experiments. The interest of the Veterans Administration and of the American Trudeau Society was at once aroused, and extensive clinical investigations, on an unprecedented scale, were begun. Streptomycin proved to be the first chemotherapeutic agent ever discovered which had the capacity of controlling the dreadful "white plague" of man. It was not a perfect agent, it had its limitations; it certainly was not a cure for tuberculosis. But it pointed a way toward the final control of this dreadful disease.

Streptomycin also opened many other new fields of chemotherapy, notably in the treatment of infections not subject to penicillin therapy. This was true of urinary tract infections, tularemia, plague, influenza, meningitis, brucellosis, and many others. Although later other antibiotics were found that could replace streptomycin, it pointed the way to the control of numerous diseases not previously subject to treatment.

The Road to Streptomycin and Beyond

IN 1939, I started on a new type of hunt, one of the most peculiar and exciting that man has ever undertaken—a hunt for microbes that have the capacity to yield a new kind of life-saving drug. How can expeditions for lions and tigers compare, in the nature of their preparation and results achieved, in the anxiety due to the many failures encountered and in the joy of the final accomplishments, with this new form of hunting? How can a mere desire to kill wild animals, whether they be birds or rabbits, crocodiles or behemoths, compare with the search for microbes which might yield chemical substances that have the capacity to combat such devastating human diseases and epidemics as tuberculosis and bubonic plague, intestinal infections and typhus fever, typhoid and brucellosis, leprosy and syphilis? The human mind can visualize a hunter trying to kill a dangerous leopard or a lion. But can it visualize the attempt of another type of hunter to domesticate unknown microbes hidden in the soil or in a manure pile, in order to combat the numerous infectious disease-producing germs that lurk to attack man and animals?

During past history, notably since the days of Louis Pasteur, the investigator has searched for causes of infectious diseases or epidemics and for agents responsible for processes of decomposition or fermentation. There have been naturalists and collectors before who have searched all the wide regions of the world for drug-yielding plants or for new varieties of plants or animals which could be domesticated by

man for the improvement of his crops and his flocks. But this new type of search was something different. This was a combination of a microbial farm, a chemical laboratory, and a battery of experimental animals all used for the purpose of determining the effectiveness of a new type of drug produced by microbes.

The stakes were high, and the game seemed worth the chance. I was fully prepared to undertake such a study as a result of my intimate knowledge of fungi, actinomycetes, and bacteria, their occurrence in soil and their physiology. The methods that we worked out for the occurrence of the organisms in the soil and their activities could easily be modified to fit the new problems.

Numerous questions presented themselves, however. Did I possess sufficient laboratory and other facilities to do a proper job on this new and difficult problem? How could I, working in a small laboratory, which had previously been devoted primarily to soil microbiological problems, expect to uncover hidden secrets that would tend to influence medical science and medical practice? I had only limited chemical facilities to attack a problem that was not only microbiological but also to a considerable extent chemical in nature. This type of problem required extensive animal experimentation to answer the various questions of potential toxicity of new preparations that we were going to isolate and to determine their activity in the animal body as compared to their action in the test tube. Such investigations were bound to involve large-scale production of antibiotics, in order to obtain adequate quantities for chemical, animal, and finally clinical experimentation.

It took considerable courage, perhaps even more naïveness, to attempt a problem of this nature. Looking back to that period, I now realize that it was almost presumptuous on my part to undertake this type of investigation. I felt, however, that I was prepared to handle the various aspects that might arise in connection with the study of the microbes. Since a large part of my doctorate training was in biochemistry and chemistry, I knew at least the chemical approach to the problems in question. There were available at the Experiment Station two departments of animal and poultry husbandry, where I could no doubt obtain some help for animal testing. Moreover, my close relations with Merck & Co. carried with it potential collaboration in the chemical isolation and purification of new compounds, their large-scale production,

and the evaluation in animals of their chemotherapeutic potentialities.

During the many years of my digging in the soil and in the manure compost, of searching for unknown microbes, I had thought of the fate of all disease-producing germs, whether they be fungi or bacteria, protozoa or viruses, whether they attack the human or animal body, big plants or small. They all find, sooner or later, their way into the soil. This occurs either in the form of human or animal excreta or in the dead bodies of the deceased individuals. What becomes of all these germs? Since they tend to disappear rapidly in the soil, the question was whether this is because they are unable to live in the soil, or because they are destroyed by the other soil microbes. If the latter were the case, to what extent could such microbes be utilized for the purpose of producing chemical substances which would have a similar effect upon the pathogenic germs, in culture or even in the human or animal body?

These or similar ideas had been uppermost in the minds of many investigators beginning with that of Louis Pasteur some three-quarters of a century earlier. When he saw that a group of sheep infected with the deadly anthrax failed to develop the disease simply because the culture of the anthrax organism was contaminated with a harmless bacterium, he prophesied that "the time may come when we may utilize harmless microbes for combating harmful ones."

These ideas were certainly in the mind of Dr. O. Avery of the Rockefeller Institute, when he asked me in 1927 to recommend to him a young man trained in the study of soil microbes, so that an attempt could be made to isolate from the soil an organism capable of causing the destruction of the pneumonia-producing germ, the pneumococcus. I recommended René Dubos, who had just completed three years of training in my laboratory where he was engaged in the study of a strictly soil process, the decomposition of cellulose by bacteria. When put to the task, he at once visualized the significance of the problem before him. Within five years, he succeeded in isolating from the soil a bacillus that produced an enzyme system which had the capacity to dissolve the capsule of the pneumococcus germ, thus making it sensitive to destruction by the white blood corpuscles in the human body.

This was in 1932. The same year, I undertook, at the request of

the National Research Council and the Tuberculosis Association, to direct a project on the fate of tuberculosis germs in the soil. I assigned this problem to one of my students, who, after three years, demonstrated, as had many before him, that this germ does not remain alive for very long in the soil or in water basins.

In the meantime, I undertook myself, together with some of my other students, a comprehensive study of the effects of one microbe upon another when both are living in close association. The contemplation of the nature and activities of the microbial population of the soil gave much food for thought concerning the tremendous potentialities that these activities offered for a better understanding of soil processes and possibly the effect of soil microbes upon disease-producing organisms. Who could have had a better opportunity to contemplate such potentialities? Having wrestled for more than two decades with the problem of complex microbiological life and the manifold activities of microorganisms in the soil, in the manure pile, in the sea, and in the peat bog, I fully recognized the fact that microbes do not live in watertight compartments in these habitats, that, unlike those that cause human and animal diseases or the fermentation of beer, wine, or vinegar, they do not live in pure culture. On the contrary, in the soil and in the manure pile, they live in complex mixtures. How do they thus affect the life and growth of one another?

These and other questions kept puzzling me, and, at the end of 1936, I presented some of these ideas in a series of papers, in which I tried to summarize the knowledge existing at that time concerning the effects of some microbes upon others. I tried to draw certain broad conclusions, largely applied, however, to soil problems. The time was not yet ripe for me to undertake in full earnest the study of disease-producing organisms and their relation to soil microbes, although I had another student undertake systematic studies of the changes in the soil population as a result of enrichment with specific bacteria. The opportune time came about two years later when Dubos succeeded, in 1939, in demonstrating that by systematic study of the soil microbes, it is relatively easy to isolate microbes which are able to kill disease-producing germs, grown in pure culture on a large scale, that such cultures will yield chemical substances which have the capacity to kill the deadly germs not only in the test tube but also in the animal body, and

that such substances could be utilized as chemotherapeutic agents for the control of certain human and animal diseases.

One may argue that there was nothing new in that scientific contribution. Every step was already known, notably the enrichment of soil with disease-producing bacteria, the isolation from the soil of microbes effective against such bacteria, the cultivation of such microbes in the laboratory, the isolation from their cultures of chemical substances effective upon various pathogenic bacteria, and the final action of such substances on the pathogen introduced into the animal body. This is all true. But to obtain the desired results required an analytical mind, an original co-ordination of all the facts, and especially a new philosophy, which reasoned out and combined these steps into a successive and successful sequence.

One can further speculate on the fact that the particular substance isolated by Dubos, gramicidin, or tyrothricin, as it came to be known later, did not prove to be an ideal therapeutic agent, that neither in its effectiveness nor in the range of its activity against bacteria could it compare with penicillin or with streptomycin, which soon followed. But it was the beginning of an epoch, it pointed a way, it opened a new approach. Pyocyanase, discovered nearly forty years previously, also a microbial product, also active on disease-producing bacteria, both in the test tube and in the animal body, no longer attracted serious attention, because of lack of appreciation of the fact that here was a new field of science that was to yield great discoveries for alleviation of human ills. Alexander Fleming, a keen observer, discovered and named penicillin ten years previously; this, as well, had to wait its turn to find practical application, remaining all these years a mere laboratory curiosity, like many other microbial products isolated previously. Numerous other substances produced by bacteria and molds were known even prior to this work, substances that had the capacity to destroy deadly germs; they were being discovered almost every year, from mycophenolic acid in 1895 to gliotoxin in 1935, both formed by fungi. Certain products of bacteria, equally effective, also were known.

There was needed a synthesis or a co-ordination of all these ideas. This came in 1939. The time was now ripe. Certainly it was ripe for me, if not for many others. Here were not only new facts, but a new approach. In my many discussions of this and similar ideas with

Dubos during 1938 and 1939, as he was working on this problem, I became fully convinced that all my prior knowledge of the fungi and actinomycetes, of their occurrence and activities, gave me just the tools required for this type of approach.

Who can forget the excitement of those early days? It was like immersing oneself in a whirlpool of new ideas, new facts, new methods, new interpretations, and new applications. Each new experiment opened numerous vistas for others to follow. Each newly isolated compound pointed to many others, more effective, less toxic. It is hardly possible to tell this story in full detail. Some of the developments have been noted elsewhere in this book. Others have been recorded in the numerous scientific papers from my laboratory, both by myself and my students, and in the many popular and scientific addresses that I was soon called upon to deliver. Only a few of these events can be recorded here.

The staff that I assembled for this study in 1939 consisted, in addition to myself, of three graduate students, one of whom was Boyd Woodruff, one assistant, and three visiting investigators. We proceeded to compare various representative cultures among the bacteria, fungi, and actinomycetes for their ability to produce chemical substances that had a growth-inhibiting effect upon various bacteria, including disease-producing forms. We came first in contact with the pyocyanous organism that produced two known substances *pyocyanase* and *pyocyanin*. Although these did not prove to be very desirable for disease control, they enabled us to develop satisfactory procedures for testing purposes and methods for the extraction and purification of the active substance.

Whether it was sheer luck or my previous familiarity with the group of microbes, the next culture that came immediately to our attention was an actinomycete. Although the particular culture was different from those already known to me, I was thoroughly familiar with the methods for its cultivation. We utilized the same procedures for the isolation and testing of the active substance as we had developed for pyocyanase. The new material was found to be a red pigment, soluble in organic solvents, and highly toxic to animals. We named it actinomycin. As a result of close collaboration with the chemists and pharmacologists at Merck & Co., we established its chemical nature and action in the animal body.

The problem I now outlined in 1940 involved a tremendous amount of detail work. Thousands of cultures of microbes were to be isolated from soils or from other natural materials. These microbes were to be grown on various media or substrates. They were to be tested for their ability to produce, under different conditions of cultivation, chemical substances which would have the capacity to inhibit the growth of various bacteria, pathogenic and nonpathogenic. The promising microbes were to be further purified, selected, and retested under different conditions, to obtain the maximum yield of the desirable compounds. These would be isolated from the cultures of the microbes by various chemical procedures, then further purified and concentrated. They were to be tested for their activity against bacteria and other microbes. Their toxicity to animals would be established. Their effect on disease-producing germs in the animal body would then be determined before their potential properties as therapeutic agents could be evaluated. Finally, arrangements would be made for testing the more promising agents clinically for their ability to control infectious diseases in the human body.

These many steps involved in the isolation of new chemical compounds that possessed therapeutic potentialities could hardly be appreciated by those outside the enchanted circle. Numerous disappointments were bound to result. One could hardly say that we were very lucky in the immediate isolation of promising substances. Following actinomycin, we succeeded in isolating streptothricin. This proved to be a much more promising compound, since it was not very toxic and possessed highly desirable antibacterial properties. Still it had certain limitations. We were on the way, however. All that we needed was to isolate compounds similar to streptothricin but somewhat less toxic. This led to the isolation of streptomycin, which proved to be a desirable agent for the treatment of many infections not previously subject to therapy. Later, we isolated other antibiotics, none of which has so far superseded streptomycin, although some, like neomycin, possess highly desirable chemotherapeutic properties and found an important place in the treatment of many infectious diseases.

In my public addresses, I was often asked whether streptomycin was isolated in our laboratory as a result of observing a contaminated

bacterial plate (it is to be recalled that this was the case of penicillin, isolated by Sir Alexander Fleming from a green mold, which he observed as a contamination on a bacterial agar plate where the bacteria did not grow around the mold colony). My usual reply was as follows: "No, not quite! We went about it the hard way. We isolated freshly some ten thousand cultures of different microbes. These were tested for their activity against bacteria. Ten per cent of them were found to possess such potentiality, thus giving us a total of one thousand active cultures. The latter were now grown on various liquid media, to find those which had the capacity to liberate freely the substances possessing such activity; ten per cent were found to yield such substances. This gave us one hundred promising cultures. Since the active substances produced by these cultures were unknown and since they all required different chemical manipulations for isolation and purification, we finally succeeded in developing procedures for the isolation of ten per cent of them, thus giving us a total of ten new compounds. When these were tested for their therapeutic activity in animals, some were found to be too toxic, others were not very active in the body or did not possess the desirable kind of activity. Only one of these proved to be a successful agent, streptomycin."

This, of course, is only a story, since by this time we must have isolated and tested some one hundred thousand cultures; still, it is close enough to the truth. Among the various microbes that my assistants, students, and I isolated and tested for their ability to produce antibiotics, the actinomycetes proved to be the most fruitful. They yielded in our hands, and later in the hands of other investigators in numerous university and industrial laboratories, some of the most interesting and most promising antibiotics, about ten of which have now found extensive application in human and animal therapy, and, more recently, in the nutrition of various animals.

To carry on this extensive project of isolation, selection, and testing of different microbes, as well as the production and purification of the active chemical substances, I needed more financial assistance than the Experiment Station was able to provide me with. One assistant was paid from university funds; one or two additional graduate students had come to work for their advanced degrees and were paid from in-

dustrial fellowships; one or two visitors had come to learn new techniques, and I was able to utilize them in connection with some of the investigations involved in the new problem.

Conditions were even more critical after Pearl Harbor, when it became difficult to find properly qualified assistants for the meager salaries we were able to offer. Additional help was badly needed, if I were to pursue this problem in a satisfactory manner. With this in view, I applied to the Committee of Medical Research, which had just been set up in Washington by one of the government agencies for the purpose of supporting various kinds of research in the field of medical science bearing upon the war effort. I received from that committee a courteous reply, which read somewhat as follows: "We are now at war. All our efforts must be directed toward winning this war. What you propose to do is of purely theoretical significance and does not promise immediate practical results. Therefore, your application cannot be approved by this committee." Thus a project which was to prove of tremendous benefit to mankind in alleviating human diseases, including infections resulting directly from battle casualties, was turned down as of little promise from a practical point of view. I understand that in the Korean War, largely because of the proper use of penicillin, streptomycin, and other antibiotics, there were very few deaths from infectious diseases.

Fortunately, the chairman of the committee, Dr. A. N. Richards, was a keen and farsighted person. He fully appreciated the potentialities of the project I proposed. In forwarding to me the above decision, he asked whether I would approve his sending my application to a private foundation, which might be interested in supporting this type of investigation. To this I heartily agreed, and within a week I received a visit from a representative of the Commonwealth Fund, to whom I presented in greater detail my plans for the development of a comprehensive program on antibiotics. Two weeks later, I was informed that my application had been approved and that an annual grant of \$9,600 had been allowed for my work. This grant was to continue for six years. Later, Mrs. Albert Lasker offered to give us additional financial support, thus making it possible for me to develop a broad screening and isolation program of antibiotics.

We thus succeeded in obtaining the necessary assistance from pri-

vate foundations where the government agencies had failed to recognize the merit and potentialities of the proposed project. This was largely the reason I never again asked any government agency directly for financial help for our work. The federal government is said to have spent about \$600 million in assisting in the development of the penicillin program in America. Streptomycin, which proved to be almost as important in saving human lives as was penicillin, did not cost the federal or state governments one single cent. The development of the antibiotics program in our laboratories was solely financed by the meager facilities of the Department of Microbiology of Rutgers University, with some additional help from industrial organizations and the two philanthropic agencies previously mentioned.

Late in 1941 and early in 1942 it became apparent that we were dealing with a new field of science and that many new chemical compounds were about to appear, all of them possessing certain common properties. This had begun with gramicidin in 1939, with the rediscovery of penicillin in 1940, and with our own isolation of actinomycin the same year. These were soon to be followed by a number of others. At the request of an editor of an abstract journal in 1941, I suggested the word "antibiotic" as an all-embracing term. In doing this, I made use of an old adjective, which was at one time used to designate the injurious effect of one organism upon another, as one preying upon another; in this respect the word had never come into general use. My suggestion was immediately accepted and received universal recognition. Before long, physicians spoke of "antibiotic therapy," pharmaceutical companies were organized as "antibiotic societies," and new journals dealing with "antibiotics" were started.

As I look back to the rapid development of this new branch of science which may be said to have started only about fourteen or fifteen years ago, although its roots go much farther into the historical background of the development of our knowledge of microbes, I am amazed at the great progress that has been made in this brief period of time. The discovery that saprophytic, or "good," microbes were capable of producing chemical compounds that had the property of destroying disease-producing, or "bad," microbes, and the further discovery that these compounds could be utilized by the medical profession for the treatment of numerous infectious diseases never before subject to ther-

apy, appealed greatly to popular imagination. Here were the miraculous substances that would do away with infections and epidemics! No wonder the public gasped in amazement at these compounds and at once designated them as "miracle drugs."

The horizons were now unlimited. New fields were opened. Was it not time to attack the most difficult of all infectious diseases, tuberculosis? My interest in this problem was developing slowly, stage by stage. Early in 1941, Dr. W. H. White, of the National Tuberculosis Association, invited me to attend a small conference which he called together in New York to discuss chemotherapy of tuberculosis. This conference was attended by representatives of several industrial organizations and some tuberculosis experts. Dr. White emphasized that the time might be ripe for undertaking a systematic search for chemical agents which would be effective against tuberculosis. He spoke particularly of certain enzyme mechanisms, such as the digestive processes in earthworms. I could not agree with him in this type of approach. When pressed for suggestions, I replied, "The time is not ripe yet, but it will come through the antibiotics."

The isolation of streptothricin that year provided new food for thought. But still I did not feel ready to make a concerted attack upon the tuberculosis problem. More information was still needed on the mechanism of the action of the known antibiotics and new antibiotics had to be found. We still had to wait. There were still certain questions of methods that had to be cleared up.

Just about that time, I received a very interesting letter from my own son, written in May, 1942. Byron was at that time completing his training in bacteriology at the University of Pennsylvania Medical School, where he was a student. He was looking around for a suitable problem that he could make a subject for investigation in the limited time available to him during his junior and senior years. He wrote to me as follows:

In reading the reprints you sent me, I was struck again with the urge to do some work in the direction of finding an effective *in vivo* antagonist to the tubercle bacillus. I was particularly impressed with the relative simplicity of the method you have used in isolating fungi-producing antibiotic substances, and I wondered if exactly the same method could not be used with equal ease to isolate a number of strains

of fungi or actinomycetes which would act against *M. tuberculosis*. They could be tested after isolation against some more rapidly growing organism such as *M. phlei in vitro*, and finally against the tubercle bacillus itself *in vivo*.

I hope you will tell me exactly what you think of this problem. From the little reading I have done, it is my impression that no one as yet has published any work of this nature. There is no question that it has a great deal of practical value or would have if successfully concluded.

If you do approve of my idea for a problem and would like to talk it over with Dr. Sevag and me, let me suggest June 25 or 26 as an ideal date for such a discussion. It is probably the only occasion that will be available for such a trip, providing I follow my vacation as planned.

As I reread these lines, I feel a pang of regret. Had I followed my son's suggestions, had I in the least encouraged him, his own future career might have been affected, and I would have saved myself certain unpleasantness later. Unfortunately, it is much easier to look backward than forward. At that time, I was still not ready. Another year had to pass before my whole philosophy of the antibiotics became clarified, before I was ready to attack this most difficult problem. I replied to Byron, therefore, quite simply:

The time has not come yet. We are not quite prepared to undertake this problem. But we are rapidly approaching it.

Finally, I was ready. This was early in 1943. I decided to approach this problem from two different directions. First, to utilize a method made famous in soil microbiology, by enriching the soil with living and dead cells of the tuberculosis organism, in order to favor the development of microbes which might have the capacity to produce antituberculosis agents; second, to use nonpathogenic strains of the tuberculosis organism, as suggested by Byron, for the purpose of testing the activity of newly isolated cultures of microbes. With this in view, I asked Dr. Florence Sabin of the Phipps Institute in Philadelphia to supply us with fresh cultures as well as with a quantity of dead cells of the tubercle bacillus.

The cell material was used to enrich soil for the purpose of isolating antituberculosis organisms. Soon after the discovery of strepto-

mycin, we demonstrated the ability of this antibiotic to inhibit growth of the tubercle bacillus. A similar property of streptothricin was later demonstrated in another laboratory. Thus, we came to recognize the fact, now well established, that various antibiotics produced by actinomycetes are capable of suppressing the growth of the tuberculosis organism. This is now known to be true of a number of other antibiotics, such as neomycin, viomycin, and oxytetracycline. Of all these antibiotics, streptomycin occupies the leading, if not the only important, place.

These are just some of the facts behind the scenes of the discovery of the effectiveness of antibiotics against tuberculosis. I am telling this now to emphasize how various ideas and facts had been influencing, guiding, and stimulating the search for new antibiotic agents, especially those active against tuberculosis, each doing what the previous one did not. Our laboratory played an important role in this discovery.

With the isolation of streptomycin, it was at once recognized that we possessed here a chemotherapeutic agent which, next to penicillin, was bound to revolutionize medicine. It was soon demonstrated that streptomycin was highly effective against numerous diseases not previously subject to therapy, such as brucellosis, plague, colon infections, rabbit fever, and others. Its greatest potentialities were found to lie, however, in its capacity to suppress one of the oldest and most vicious enemies of mankind, tuberculosis. The first tests made with streptomycin in our laboratory early in 1944 definitely established that it was active against the tuberculosis germ in the test tube. Soon after, Drs. Feldman and Hinshaw of the Mayo Clinic demonstrated that it was also effective in guinea pigs infected with tuberculosis. This was followed within a few months by the first clinical investigations, in which the ability of streptomycin to combat the disease also in human beings was emphasized. Within less than three years, the medical profession became fully convinced that here was a new drug that could finally be used in suppressing if not controlling tuberculosis.

In order to be informed of the clinical properties of streptomycin, I tried to keep in touch with many of the investigators who were testing it in the laboratory and in experimental animals and with many of the clinics where it was used in the treatment of different infectious diseases. When the effectiveness of streptomycin was established first

in experimental, then in clinical, tuberculosis, general excitement began to increase. The fact that the antibiotic caused certain unfavorable reactions in some patients, especially upon the hearing and upon the vestibular function, led me to question the clinicians carefully and to read their reports attentively. I was not very happy about the general enthusiasm, which was so great that little attention was paid to the accompanying reactions, which were believed to be only minor in nature.

When, in time, streptomycin came to occupy an important place in chemotherapy, when the demand for it throughout the world increased at a far greater rate than it could be manufactured, when thousands of patients began to benefit from it, when especially sufferers from such diseases as tubercular meningitis had a thirty-five to seventy-five per cent chance of recovery, as compared to none previously, the side reactions began to attract increasing attention. This began to arouse a certain degree of pessimism among some clinicians. My friend Emil Bogen of California compared the prevailing attitude toward streptomycin in 1946 to that of an optimist and a pessimist. The optimist says: "Look at the fifty to seventy-five per cent of the patients who benefited from its use!" The pessimist insists: "Look at the twenty-five to fifty per cent who have not benefited!" That is human nature, I suppose. People take the words "miracle drugs" literally: they expect such drugs to perform nothing but miracles.

As late as 1947, even after I had personally witnessed many cases of tuberculosis which were helped if not completely cured by streptomycin, after I had talked to many clinicians who spoke of its wonderful effects in tubercular meningitis, in galloping tuberculosis, in tuberculosis of the larynx, and even in pulmonary tuberculosis, and after I was assured that many of these patients would have been dead were it not for streptomycin, I still felt rather uneasy, I was still afraid that something might turn out to be wrong somewhere, that the toxic reactions and problems of resistance resulting in the recurrence of the disease might obliterate any potentialities that streptomycin might have for the treatment of the disease.

If I could only describe the touching scenes I have had many an opportunity to observe in which antibiotics, especially streptomycin, virtually brought dying people back to life! Take, for example, the

one I witnessed in 1946 in Moscow, at the Children's Hospital, where Ninotchka, a little girl barely nine years old, the daughter of a famous mathematician, was sitting up in bed having her breakfast on the eighty-third day of her admission to the hospital. She should have died nine weeks earlier from that deadliest of microbial killers, tubercular meningitis, which never spared any of its victims. The doctors surrounding Ninotchka looked upon her in awe as upon a child Lazarus. Here certainly was a wonder which none of them had ever before witnessed. Out of four hundred children afflicted annually by this disease and admitted to this hospital not one had ever survived, not one had ever left the hospital alive. Here was the first case of a child brought back to life, so to speak. A great painter would be needed to do justice to that scene of doctors and nurses surrounding Ninotchka's bed. She had been taught a poem in English, in honor of our visit. Setting aside her breakfast, she recited it, to the joy of her father and the wonder of all the others around her.

Could I but tell of groups of children in many of the Italian cities—Rome, Florence, Verona—as well as in some Spanish cities that we visited! A large number of those afflicted by tubercular meningitis and miliary or generalized tuberculosis had recovered because of streptomycin. They came to greet us as we passed through those cities. Singing and dancing, and surrounded by their parents and teachers, they showed their appreciation in various ways. Throughout the whole human history of tuberculosis, prior to the advent of streptomycin, only fifty cases are said to be on record of patients suffering from tubercular meningitis who have recovered from this disease, as Professor Caronia told me in Rome. But even these cases may not have been diagnosed properly. Now, however, more than fifty per cent of all those stricken with this disease are able to recover.

What about all the great men and women of all times, musicians and scientists, writers and artists, who were stricken in the past by the "great white plague"! How many of them would have lived many more years and been able to do even more productive work had streptomycin come sooner! "I had six sons," the poetess Justa Mata of Spain wrote me, "three of whom died from tubercular meningitis and the other three from pulmonary tuberculosis. Had you come into this world earlier, some of my sons might have been alive today."

Of the numerous letters I received, all expressing appreciation, and some hoping that we were finally on the threshold of combating one of the most dreadful diseases of mankind, the following were particularly touching:

We patients in ward C-5 at the F. General Hospital, located in D., read with much interest the article appearing in *Time* magazine the week of January 29, 1945, concerning your search for an antibiotic that will cure tuberculosis. We have all contracted this disease while in the service of our country, almost all of us overseas. There are over thirty of us in this ward waiting either to die or for TB to run its course. We know we have the best doctors and care here, but waiting and resting is much harder than any battlefield or beachhead. We at ward C-5 cheer you on to a quick victory in your search for a cure for our disease. Respectfully, Sgt. W. L. T.

This should be no ordinary letter of thankfulness to you, for it is wrenched from the souls of a father and a mother who saw their once hale and hearty sailor lad go down to certain death had it not been for the saving powers of the drug you brought to use, through your devotion to alleviating the ills of humanity. Looking at our son's embodiment of health as a "boot" at Great Lakes—eighteen years old, 185 pounds, six feet two inches tall—it overwhelms us to have brought forth so strong and nobly dispositioned a son. To go through in imagination his overseas service on a duty destroyer in the Pacific up to his being taken with pneumonia and empyema during combat off the shores of Tokyo; his journey home from island hospital to island hospital; his Christmas home leave from Great Lakes Hospital; and then on the eve of a medical discharge to see him struck with the dread one hundred per cent fatal disease of TB meningitis—is again overwhelming. His father and I truly went down with him into the Valley of the Shadow, but because of you there was piercing that Shadow that ray of hope that caused a pulsing word to beat in our hearts and minds—streptomycin, streptomycin, streptomycin! Friends, relatives, everyone—told us they were praying for Bill's recovery and for us. God answered their prayers long before they knew they were needed when men like you were inspired to devote their lives to bettering the welfare of others, through research like yours. Looking at Bill now at nineteen years—in his hospital bed—white, very thin, 130 pounds, but again smiling at us and talking with us, hopeful and cheerful, we are again

overwhelmed at his having been given to us a second time. It may be early for scientists to say his is a complete recovery and that he will be as well and strong as he started out to be, but we parents are overjoyed to have the chance to hope he will build up and become the fine man he promised to be. We cannot wait for weeks or months of convalescence to pass and prove more facts before we try to tell you about our gratefulness to you and to acknowledge our great indebtedness. We wish someday we could see you or speak to you, befriend you, entertain you in our little home on our peaceful lake or in some way repay a fragment of the debt we owe you. Sincerely yours, Eve L. P.

This came from a prominent priest who was afflicted with tuberculosis and whom I visited in his sanatorium:

I entirely forgot the thing that has been uppermost in my mind—and that is to thank you for your gift of streptomycin to me and to many other sufferers. I am deeply grateful to you for the means of regaining my health which you have provided. God bless you, Doctor, for your patient, self-sacrificing contribution to suffering humanity. Sincerely, Father H. A. St. P.

This was just four years after the effect of streptomycin in tuberculosis had been established. This effect could best be illustrated from the reports of the Veterans Administration, which has been holding, since 1945, semiannual and, after 1950, annual, conferences for the purpose of careful evaluation of the effect of streptomycin in tuberculosis. The results of a large number of Veteran and Army and Navy hospitals had been analyzed in detail, and the effect of streptomycin on tuberculosis had been compared with that of various other drugs. The general conclusion was reached that this is the first time in the history of the "white plague" of mankind that a drug has been found which is effective in the suppression of this dreadful disease. To be sure, streptomycin was shown from the first not to be an ideal drug; it certainly could not be considered as a cure for all forms of tuberculosis. Still it has been effective in saving many lives. It has opened a new field in therapy, it has pointed a way toward the final solution of tuberculosis, whereby this disease as well as syphilis and pneumonia will pass into the limbo of history. Few people now die of epidemics that formerly ravished mankind. What has become of plague, small-

pox, cholera, typhoid fever, gonorrhea, and others? Tuberculosis appears to be headed in the same direction.

When we announced the isolation of a new antibiotic, neomycin, early in 1949, in the hope that it might supplement streptomycin in its effect upon tuberculosis, another flood of letters began to arrive, repeating what happened in 1944-1945.

One father wrote to me from France:

I had two sons, both of whom contracted tuberculosis during the last war. One died in 1945, just on the eve of streptomycin's becoming available. The second son is sick now, but he does not respond to the effect of streptomycin. Is it possible that he will die just on the eve of a new drug's appearing for combating this dreadful disease, just as my other son did!

Whenever I addressed scientific and popular audiences, I often received a similar response. I, who was trained to work with the smallest and humblest of living things, the microbes, notably those that live in the most natural of environments, the soil, was suddenly catapulted into the public eye. It became difficult for me now to pass unrecognized, it became difficult to continue leading my own simple life. People have come to expect too much. "You have given us an agent for combating tuberculosis—now we expect you to find something for the treatment of cancer, and you will," said a prominent businessman, following an address that I gave in his industrial city. Little did he know, little did he appreciate the labor that goes into it. The search, the constant search! The many disappointments, the sleepless nights, the worries, and the many failures!

One tries to follow a plan, a system. Does every search of fundamental principles result in practical applications? It is easy enough for an outsider to ask whether the work in our department, which appeared to be at first fundamental, has turned to the solution of practical problems, and if so, whether such developments cannot in their turn yield funds for the support of fundamental research. One might, therefore, ask the logical question, what is fundamental and what is applied research? Was I doing only fundamental research when I studied the soil microbes for a quarter of a century, without arousing a ripple of interest, simply because my work had little prac-

tical application? During that time I wrote many scientific papers and published many books, but these came to be known only to a few specialists or, worse yet, remained to gather dust on library shelves. During my previous twenty-five years, before the advent of antibiotics, I isolated many new organisms and named a number of them. I examined their activities, I postulated various theories concerning their potential role in nature, without receiving so much as a word of encouragement from anyone. I could almost hear people say: "Why is he wasting public money all these years? Where is all this research going to lead him, or us, for that matter?" Did not a prominent scientist in Washington say that he knew of no branch of science that had yielded so little information of practical value as had soil microbiology? What difference does it make whether a few more organisms are isolated and described; you certainly cannot know all about these millions of microbes.

Has my work, done in the same laboratory and in the same environment, after 1940 suddenly become applied, simply because it has found application in the treatment of infectious diseases? Again, is an unsuccessful experiment, like the isolation of actinomycin, a highly interesting antibiotic which unfortunately proved to be very toxic, more fundamental in nature because it has found no practical application, or is it less applied than a successful group of experiments, like the isolation of streptomycin, which has proved to be of great practical value?

A person can hardly differentiate between the fundamental and the applied, between the theoretical and the practical. Where does the former end and the latter begin? Is it not merely the point of view? One scientist is prepared for or cares more for one method of approach than for another and looks for information and theories that will substantiate one or the other. Usually a scientist is most happy when the results of an experiment, over which he has pondered and on which he has labored for many days and many nights, perhaps years, are successful and he has contributed to the sum total of knowledge something new, perhaps a new fact, a new idea. But why not consider an experiment that has resulted in practical applications on a similar plane as of as great scientific significance as the theoretical

experiment which serves to elucidate certain fundamental problems of living matter or the universality of life?

It appeared to me as a rather simple conclusion, from my knowledge of the potentialities of fungi and actinomycetes living in a mixed population in the soil or in the compost, that pathogenic bacteria can be readily destroyed by these microbes and that the latter possess a number of mechanisms which are involved in these reactions. I knew of the fragility of pathogenic bacteria as compared to the saprophytic forms, and that bacteria in general are more sensitive to various adverse conditions and chemical agents than are fungi and actinomycetes. Had I not, beginning in 1915, personally made numerous determinations of the relative abundance of fungi in the soil by the mere acidification of the agar medium, thus completely repressing bacterial development? Had I not observed, as had many others, that colonies of fungi or actinomycetes growing on bacterial agar plates are surrounded by halos or zones in which bacteria do not grow?

It was merely a new chapter which was to be added to my previous fund of knowledge concerning the various groups of microbes inhabiting numerous natural substrates and their effect upon one another. There was one exception: whereas previously I devoted all my attention to the microbes inhabiting the soil, the compost, or the sea basin, I was now to examine the effect of these microbes upon disease-producing bacteria. The change in methods involved in such studies was only minor in nature, except during the last stages when the chemical substances produced by these microbes were to be tested in the animal body. Here again, I could fall back upon my earlier work on the isolation of enzymes and on the study of the toxicity of salvarsan in the animal body done during my Takamine days, in 1918 to 1920. In spite of this preparation, I never expected the marvelous panorama that would unfold before my eyes. I appreciated at that time that any new discovery which affects human health or contributes toward the elimination of human disease was bound to arouse universal interest, hardly to be compared with the scant attention that was ever paid to my previous studies bearing upon the elucidation of other important problems which had an application only to the soil or to the sea.

Before I became aware of it, I was surrounded by curiosity seekers; I was swamped by fan mail from grateful people who had benefited from the use of the new drugs. "Antibiotics" soon became a byword.

Most of the letters were highly laudatory, expressing thankfulness and appreciation. Others began to arrive, however, which were not only critical but accusing. It appeared that everyone who ever had, or imagined he had had, a reaction from the use of streptomycin felt he must write to me personally about it and ask my help. What could I do for him? I could only refer him to his own physician. So far as I was personally concerned, I wanted to go back to the laboratory and try to find other agents which would be more effective and possibly free of reactions.

I had to exert a special degree of watchfulness that streptomycin did not fall into the hands of unscrupulous manufacturers, who would put it into all sorts of mixtures and ointments and claim a variety of cures for it. The temptation was very great.

Numerous suggestions began to arrive from all over the world offering me freely the use of a variety of remedies against all sorts of ailments, ranging from boils to cancer. All sorts of clay samples, soil samples, water samples began to arrive from places as far apart as Japan and Tanganyika, with requests that I examine them and isolate the elixir of life that would finally put an end to all diseases.

Invitations to give lectures began to pour in in a steady stream, at the rate of one or more every day. They came from various parts of this country and from numerous foreign countries with ideologies as far apart as those of the USSR, on the one hand, and those of the Vatican and of Spain, on the other; from Japan, at the one extreme, and Germany, at the other. I could at best accept only a small number. Thus came about my many trips to various European and Asiatic countries. Each of them involved lectures and conferences, visits to laboratories and clinics. Although I refused to see private individuals, who came for advice and for help, many actually forced themselves upon me. I thus came to appreciate fully the depth of human suffering, and the urgent need to relieve it.

Europe after the Second World War

THE field of antibiotics was developed and found extensive application in chemotherapy during the years 1939–1946, largely in the United States and to a limited extent in Great Britain. As soon as the war was over, it began to receive considerable attention throughout the world. The tremendous success of penicillin and streptomycin, and to a lesser extent of tyrothricin, bacitracin, and the promise of several other agents in controlling numerous infectious diseases that had not lent themselves previously to therapy aroused great interest in the potentialities of this field of research and application. Virtually every bacteriological laboratory in the United States, and soon in many other countries, many of the biochemical and organic chemical laboratories, as well as botanical, clinical, and other research groups began now to focus attention upon antibiotics. No wonder. These agents were virtually revolutionizing medical practice.

The European research centers awakened to the latent potentialities of antibiotics. They were most anxious to learn of the accomplishments in the United States. I began to receive invitations to come to Europe to deliver lectures and to advise on research programs. Many of these invitations were highly tempting. One came from the Soviet Union, where the Academy of Sciences offered a much-coveted visa and a complete coverage of all expenses during our stay in that country. Another came from Belgium, where I was to give several lectures

under the auspices of the Belgian-American Foundation; still others from Stockholm, Copenhagen, and other centers. An invitation came from the Vatican to come for an important conference, with full expenses paid.

Although it was difficult for me to leave my work in 1946 for an extended journey abroad, I decided, after careful consideration of the problems involved, to go to Europe that year. I was most anxious to see Peisi again, as well as the remnants of Bobili's family (one brother had been killed in the war and her mother had died). We wanted to see Europe again as it appeared after the world catastrophe. Most important, we were hoping to see our son Byron in Germany, where he was stationed with the American Army of Occupation, since we had not seen him for more than eighteen months, after he left for the war front.

It took much effort to organize this postwar voyage to Europe. There was first of all the difficulty of obtaining proper transportation. Next came the problem of the passport, for the State Department was willing enough to issue a passport to me personally, but not to include Bobili. Finally that was settled. Then came the problem of obtaining foreign visas and the necessary military permit to pass through the occupied zones of Germany. Since the value of the dollar was pegged at a low rate as compared to some of the European currencies, the financial problem was also somewhat difficult. The risks involved in our traveling as ordinary tourists, even if lecture bound but without the proper backing by government or industry that would foot the bill or provide the necessary priorities, seemed almost insurmountable. Gradually and with some inconveniences, everything was arranged. We had to go by air, since our first goal was Moscow and the hazards of travel by Soviet boat were too great, involving passing either through the mine-laden Baltic Sea or via the long route through the Mediterranean and Black Seas. A news correspondent whom we knew and who went to Moscow about the same time by boat spent more than a month on the trip, which involved calling at several African and Asiatic ports before proceeding to Odessa, in place of the direct trip to Leningrad which the boat was originally scheduled to make. Constellations were being grounded at that time, and air transportation

without priorities was, therefore, most difficult to obtain. We finally succeeded in booking two seats on a TWA Skymaster.

With the aid of the National Research Council and my claim that Bobili, by reason of her better knowledge of Russian, would act as my secretary, we finally obtained a passport. The military permit involved further trips to Washington. In this I was aided by Princeton University, which entrusted me with the mission of inviting the Soviet Academy of Sciences to send delegates to participate in various scientific conferences that Princeton had arranged to celebrate its Two Hundredth Anniversary.

Early one morning in June, after waiting six days for the departure of the plane, we left for La Guardia airfield in New York. There was further delay and, instead of noon, the plane took off at six P.M. Several friends who came to see us leave became tired of staying with us, since everything that had to be said was said. They finally left us alone. We meandered around the station anxiously waiting for the announcement of the departure of the plane. Eventually we were off. It was extremely hot in the plane, and we began to shed our wraps, one after another. Although we had some previous flying experience, this was the first time we had dared to cross the Atlantic. At midnight, we arrived in Gander and were permitted to leave the plane for a few minutes and have some food. This gave us a chance to meet some of our fellow passengers. They could be divided into two categories: American officials going to Germany on one mission or another, and tourists going to visit their old homeland.

Our arrival the next morning at Shannon, Ireland, was uneventful. Imagine our surprise, however, when we were told that we would have to leave the plane and wait a few hours for another one which would take us to Berlin. We were told to take along small overnight bags and were driven to an inn, actually a boardinghouse, in the village of Adair, some thirty miles from Shannon. The few hours extended to three full days. The facilities in the inn, especially the food, were very poor. We were resented by the management as interlopers who had come to take the food away from their regular guests. There was nothing we could do about it. We were herded there like cattle and were told to wait. Had we known that we would have three full

days in Ireland, we could easily have gone off to Dublin or to see some of the surrounding country. All we could do now was to spend the days walking and the evenings in the pub, drinking Guinness.

After this most unpleasant stay at Adair, we were taken back by bus to Shannon. It was certainly pleasant to see the big sky bird waiting for us. It did not take long to cross Great Britain, land in Amsterdam, where we were allowed to have some refreshments, and take off to Germany. To our great discomfort we were told, a few minutes before our arrival in Frankfurt, the last stop before Berlin, that we would have to leave the plane here and proceed to Berlin by train or military plane; since there were only six passengers going to Berlin and there was an acute shortage of transatlantic planes, it was necessary for our plane to return to the States. We were sad indeed to think that here we were almost at our goal and must face another inconvenience.

Imagine our pleasure, therefore, when, as we stepped off the plane at the Frankfurt airport, our son Byron came up, dressed in his military uniform, to greet us. He lifted his mother off her feet, to the delight of all the airport personnel. He had been in Berlin for eight days waiting our arrival. Since his leave had come to an end, he had to return to his post in Munich. On the way, he stopped in Frankfurt and learned at the airport that an American plane was due to arrive and that we were among the passengers. Instead of the anticipated unpleasantness, our stop off at Frankfurt became a most unexpected joy. We walked through the heavily bombed city, all ears for Byron's war and postwar experiences. After a late dinner at the hotel, we had another chance to stroll through some of the streets of that once beautiful city, which we had visited in 1933 on our way from Copenhagen to Innsbruck. Now, it was just a heap of ruins. Shabbily dressed people walked like shadows among the half-destroyed buildings. Coming as we had from wealthy America, hardly touched by the war, through peaceful Ireland, we were shocked to behold at first hand the ravages of war.

Late into the night we sat in our hotel room listening to Byron's experiences. He saw us off early next morning at the airport, where we boarded an Army plane for Berlin. It was no fun to be instructed how to step out of a plane and not jump if the need should arise or

how to pull the string of a parachute. This was scarcely soothing to the nerves of a couple of inexperienced civilians. Fortunately, the Army personnel, with whom Byron had become well acquainted the day before and who knew all about our trip, were most helpful to us in arranging matters at the airport and in our flight to Berlin in a plane which had none of the comforts of a commercial plane. The company representatives were also most solicitous, as if to make up, at least partly, for all the discomforts we had suffered on this trip.

We finally waved good-by to Byron and were carried off to what was formerly the capital of a great Germany. On arrival in Berlin, two hours later, we were taken to the Army headquarters where we were given some meal coupons and were assigned a room at the American Officers' Club, formerly the Carl Harnack house of the Kaiser Wilhelm Institute in Dahlem. Here we had a good luncheon and proceeded to shift for ourselves.

Things were now easily arranged, however, largely as a result of Byron's efforts. During his stay in Berlin, he had made contact with an official in the Russian zone who was frantically awaiting our arrival, since he was receiving daily telephone inquiries from Moscow as to our fate. Completely unaware of our plane delay, the Russian Academy authorities and our friends in Moscow had expected us long before this. The Russian official informed us that he was ready to dispatch us by plane the same evening or early the following morning. Since we had traveled all this distance, it seemed ridiculous to leave at once. We were anxious to see something of Berlin and suggested that the departure take place the following noon. The official promised to come to visit us at our clubhouse late that afternoon to settle matters.

This gave us some hours of free time to see the ravished city. We telephoned immediately to an American Army chaplain, a close friend of Byron's with whom he had gone through the war and who was now stationed in Berlin. The chaplain, who had been expecting our arrival, came at once in his Army jeep and took us for a drive through the city.

What a nightmare it was! Here was a once majestic city, not a medieval treasure house like Frankfurt, to be sure, but a rather cold Prussian city, clean and beautiful, which we had come to know well

in 1924, where we had spent some delightful days in 1930 and not so delightful a day in 1935—each time on our way to Russia—now completely wrecked. The gaping walls of the once grandiose buildings! What a shambles the American and British aviators had made of the Kurfürstendamm and other famous streets! Especially depressing was the appearance of the Thiergarten. The ugly-looking busts of generals that filled its once famous alleys now served as scarecrows for the birds that might be tempted to rob the meager patches of vegetables replacing its lawns. Gone were the famous linden trees and the beautiful cafés along the park. And horror of horrors, at the very entrance to the park, near the once famous parliament so mercilessly besmirched by Göring and his Nazis in the infamous fire of 1933, stood a tall monument erected in honor of the fallen Russian officers and men, and now guarded by a Soviet sentry. When we approached the monument and addressed, in Russian, the pitiful-looking boy dressed in an oversized uniform, he was rather frightened, since he was hardly accustomed to Americans addressing him in his native tongue. When we told him that we were on our way to the Soviet Union and that I expected to lecture in Moscow and in Leningrad, his face became all smiles—Leningrad was his native city. He told us frankly that he would be most happy to leave this unpleasant position and join us on our trip.

Late in the afternoon we returned to the clubhouse to find a Russian officer awaiting us. He was trained in the diplomatic corps, spoke good English, and was happy to greet us. He had tea with us at the club. We finally arranged for him to telephone us early next morning to inform us when a Russian plane would be available to take us to Moscow.

After a brief rest in our clean and comfortable room, we were called for again by our friendly chaplain, who had arranged to have us to dinner at his headquarters. A rather cheerful American Army major was also present. Here, after a fine repast accompanied by good wines and cordials, we spent the evening exchanging news of the United States and Germany. The officer was very critical of the Russians in general and of their fliers in particular. He even cautioned us about going in a Russian plane, as he said, "Their fliers are dare-devils; they fly their planes in a sort of Coney Island way and care

little for the comfort of the passengers." This was scarcely soothing to Bobili's ears! He advised train travel. This was, of course, out of the question, since it would have been a tedious and highly uncomfortable two-day journey via Warsaw, and through much destroyed territory.

Upon returning late to our club, we found at the entrance a young American soldier, who was standing guard. We stopped to chat with him. He emphasized the survival of Naziism in Germany. He added, nodding his head at two German women standing at the club switch-board near the entrance: "And not all the Nazis are among the men, either." We had a chance to note the vehemence of these women when they kicked our luggage the next morning, believing themselves to be unobserved.

The next day found us again on the streets of Berlin, guided by the chaplain. About noon, two Soviet cars (one for passengers and one for luggage), in charge of the officer who had visited us the day before, drove up to the club. We were waiting for them. We were driven to the Soviet airfield, which was located in a much poorer section of town and generally made an impression of much greater poverty than the American airport.

We were due to leave for Moscow at two P.M. on a plane arriving from Paris and carrying some Russian delegates from an international conference which was at that time taking place in Paris. Although the plane soon arrived, we were told not to hurry, but to partake freely of the excellent fruits, smoked fish, and hot meat courses. Everyone was friendly, from the Russian peasant woman who was serving us to the various officials of the airport, some of whom joined us in our repast.

Finally, a report came that there was a Soviet general in the plane, and that he was getting impatient. Although we were still being urged to take our time with the food, we hurriedly finished our repast and went to the plane. It was a large DC-4, carrying about forty passengers. The front part was separated from the rest by a curtain; here were a couch, occupied by the general, and two chairs, which were made ready for us. The plane took off smoothly. It flew rather low so that we had a good opportunity to watch the green fields and villages. Soon we came over Poland and observed at once many of the ravages of

the war. It began to rain and the plane was shaken a bit. Bobili did not feel well and the general politely offered her his couch, which improved matters considerably. The general was very much interested in America, its technical development, and economic position.

At the scheduled time, exactly seven-forty-five P.M., we arrived at the Moscow airport. Unfortunately, a delegation from the Academy of Sciences had gone to meet us at another airport, some twenty miles away, expecting us on the regular Berlin-Moscow plane instead of by special plane. Apparently the telegraphic messages had been confused. We had to wait at the airport until information of our arrival was transmitted to the academy. Fortunately, the vice-president was still at his office and, when informed of our arrival, he rushed with his own car to take us to our hotel. Meanwhile, we had an opportunity to watch the unloading of the general's luggage from the plane into his private car. Besides the many bags, there were crates and crates of fruit and other food.

At the Hotel National we were given a suite which consisted of a large and comfortable sitting room, bedroom, and bath. Our rooms overlooked the square facing the Kremlin, so that we could watch the various activities taking place there. Later, on entering the hotel dining room, we were given a booklet of tickets which would entitle us to almost everything we wanted, from food and drinks to cigarettes. Soon after, the exhausted delegation that had gone to meet us at the other airport appeared. It consisted of an official of the academy, Prof. N. A. Krassilnikov, a well-known bacteriologist, who was assigned to me as a scientific guide for my period of stay, and a woman guide, who spoke foreign languages, to arrange for us to see anything that we wanted, ranging from daily visits to museums, theaters, and concerts to trips to the outskirts of the city. We were informed that a car was at our disposal, with a trained chauffeur.

I at once made two requests of our guides: first, to obtain for us the telephone number of Peisi, so that I could immediately get in touch with him; and, second, to find the present location of Professor N. K. Vavilov, president of the academy, to whom I would transmit the formal invitations from Princeton University. Our guides would come early next morning to help us plan the schedules for the period of our stay in Moscow. We went to bed rather late, tired out from

the day's exciting events. Our guides were at work, however, and within an hour, we received Peisi's telephone number, and a few minutes later, we were conversing together. We had not seen each other or heard each other's voice since 1935; so much had happened since then. Peisi's voice sounded quite old and depressed. How his seventeen-year-old son had been killed in the war was learned in detail later. He had volunteered in the defense of Moscow, had been wounded and placed in a house that had been converted into a hospital outside of the city. When the Russian army had to retreat, they did not have time to carry the wounded with them. The Germans burned the hospital with all its inmates. Thus perished Liovushka, whom we remembered from 1930 and 1935 as a bright-eyed youngster, the joy of his mother and the pride of his father.

The next morning found us ready to start on a three-week schedule of work and sight-seeing in Moscow. First to arrive were our guides, and later Peisi and his wife. We worked out a detailed plan for each day. The question of my lectures required further consideration. It was decided that I should give three lectures in Moscow and one in Leningrad, all on the subject of antibiotics. It was further agreed that we should limit our stay to these two cities, in order to conserve our energy and have a better opportunity to meet various people and learn what had been accomplished in the Soviet Union during the war and the postwar periods. My first lecture was to be given at the Academy of Sciences a week later, and the other two lectures at two-day intervals. This gave us sufficient time to have the lectures translated into Russian by properly trained persons and have them checked, with Krassilnikov's assistance. A good translator of scientific papers not only must know both languages, but he must also possess a scientific terminology, especially a knowledge of specific terms. This is a difficult job. It took the combined efforts of several of us to make a good translation.

I also received an immediate invitation from Dr. Vavilov to visit him at his country home, which was close to Moscow and where he was spending a few days' vacation. This would give us an opportunity to discuss the Princeton invitation, my lectures, and various other matters. Several invitations also came from different ministries, notably that of medicinal preparations, foodstuffs, and fermentations, to visit

them and, if possible, hold one or more special conferences. We decided to leave this to near the end of our stay, so we could familiarize ourselves first with the existing institutions and the various research and technical laboratories.

We spent most of the first day in our rooms, since we were swamped with visitors, who began to arrive early. First came my old friend and colleague, Prof. V. I. Issatchenko, a well-known bacteriologist, formerly director of the Leningrad Botanical Gardens and now the head of the Microbiology Institute of the Academy of Sciences. He brought me an invitation to visit his institute and discuss not only research activities, but also the organization of the Russian microbiologists preparatory to the forthcoming International Congress which was to be held in Copenhagen the next year. There were representatives from several hospitals, individual visitors, including many former friends, young investigators, and a variety of others.

A stream of people and numerous telephone calls continued to pass through our apartment during our all-too-brief stay in Moscow. An American girl, residing in Moscow and working at the Botanical Institute, brought a basket of fruit and flowers as a message of good will. A representative of Radio Moscow came to ask me to broadcast my impressions of the Soviet Union to America, in a week or so. An old lady of the former nobility came to beg for streptomycin to help save her son, who was suffering from tuberculosis. The very word streptomycin was sufficient to arouse excitement.

The famous oceanographer Dr. Schmidt, who a few years before had spent many months on an ice floe studying the north-polar currents, contracted tuberculosis at that time. As one of the leading scientists of the Soviet Union, he was certainly entitled to any help that could be given him. We were invited to a luncheon arranged in his apartment, which was rather large and luxurious as judged by the prevailing standards. Present, in addition to the scientist and his wife, were his personal physician and several other prominent guests. Dr. Schmidt appeared to be well preserved, were it not for his frequent departures to a corner where he expectorated into a handkerchief. He told us of his experiences on the ice floe, how he was removed by plane and flown to California, how he was received by President

Roosevelt, whom he greatly admired. The physician continuously interrupted with questions about streptomycin and how it could be obtained. Soon after the announcement of the effectiveness of streptomycin, I had sent to several Russian scientists, at their request, just as I had done for many other countries, cultures of the streptomycin-producing organism, as well as information for the production of this antibiotic, but the Russians had had little success in producing it at that time.

My first visit to Dr. Vavilov, president of the academy, was a memorable one. Accompanied by Prof. Issatchenko and our two guides, we traveled some thirty miles outside of Moscow to a charming estate, occupied by summer homes which had been made available by the government to members of the academy and their families. One of these, or rather a part of one, was occupied by Dr. Vavilov. He closely resembled his brother, the famous geneticist, who was out of favor with the government and who had been exiled to an unknown place where he had died. He himself was a physicist of considerable reputation.

He met us with open arms and asked me to tell him frankly what was on my mind, since I was so anxious to hold a conference with him so soon after my arrival. I told him that it was a rather delicate mission and that I hesitated to speak freely before the whole group. He invited me to do so. I immediately presented my case, which appeared to me of such great importance that I took advantage of the invitation to deliver lectures in Moscow in order to present two requests to him, as to one personifying not only the Academy of Sciences but all the Soviet scientists.

I emphasized, first, that the political relations between the United States and the Soviet Union were becoming worse from day to day. Many of us were experiencing great fear that the situation before very long would become irreparable. The only groups in the two countries that could still understand one another were the scientists. It was, therefore, highly essential that they should get together as soon as possible in joint meetings; an exchange of scientific literature should begin at once; every effort should be made to establish as close a *rapprochement* between these groups as possible. Vavilov listened care-

fully to what I had to say. He agreed with me as to the importance of this problem and promised to present it to the authorities higher up for further consideration.

What was my second question? Vavilov asked. I told him of the request from Princeton University that the academy send several delegates to participate at the various scientific conferences that were being planned in connection with the commemoration of its Two Hundredth Anniversary. With this, I handed him copies of the official invitations. I accompanied this by a personal note urging immediate consideration of this matter. This might serve as an opening to the first, or the broader, problem. Vavilov assured me that he would take steps at once to appoint such delegates. He and his family expected to return to Moscow from their vacation in about a week. He would invite us at that time to have dinner at his home, when we would be able to discuss this matter further in greater privacy. This cordial conversation lasted about two hours. We then stepped outside, where I took a number of photographs.

We were extremely busy during our stay in Moscow. My first lecture was given in the auditorium of the academy, which seated about two hundred and fifty persons. There were so many more who came to attend that several other rooms had to be connected to the loud-speaker. The second lecture was, therefore, transferred to the auditorium of the Microbiological Institute, which seated about five hundred. This as well was packed, with many outside. The third lecture was then arranged in the House of Scientists, in the center of the city, with a seating capacity of about a thousand. It was also full. There were always present in the audience several of the ministers of state, many prominent investigators, especially those working in the biological sciences, numerous doctors, and students. The chairman of the meeting was the genial Prof. Orbelli, one of Pavlov's most brilliant students and now director of the Pavlov Institute in Koltushi, near Leningrad, and secretary for Biological Sciences of the Academy. He was always dressed in naval uniform, since he was also professor at the Naval Academy.

After I delivered the first lecture, the editor of the academy's publishing house came to me with a suggestion that my three lectures be published in book form. As special compensation I received fifteen

thousand rubles, a sum equivalent, at the official rate of exchange, to three thousand dollars and, at the black-market value of commodities, to an equivalent of about five hundred dollars. Since all our expenses were covered during our stay in the Soviet Union, we had no particular use for additional money, unless we could buy something that we could take along with us. We visited some of the picture galleries in the city and selected a typical Russian landscape. Its price was somewhat greater than the sum that we possessed. By leaving the frame behind, however, we could get the painting. Permission was granted to take it with us. The publication of my lectures was carried out very rapidly. Within a week we were reading proof, and, on the eve of our departure from Leningrad, I was handed twenty-five copies of the booklet, neatly printed and well bound.

In addition to my formal lectures, I was invited to attend a special conference arranged by the ministers of food and confectionary industries. Preceding this conference, the ministers invited me to their private office, where, in the presence of four or five other specialists, I was asked to tell them frankly why they "failed to produce penicillin in the Soviet Union." Since by this time, I had had ample opportunity to visit the various laboratories concerned with the study of antibiotics in general and penicillin in particular, I could have probably made a proper diagnosis. Unfortunately, such a frank answer would have undoubtedly involved certain individuals whom I could not afford to place in jeopardy. Although I found everywhere numerous young and enthusiastic scientists, many others were too self-confident or were not properly trained for the task. To their greatest misfortune, they had not appreciated the importance of penicillin when it was being developed in the United States and Great Britain, in 1942-1943, or at a time when the relations with the Soviet Union were most cordial and when they could have obtained everything free had they only asked for it. Now, when they began to appreciate its importance, it was too late to obtain the necessary information either gratis or at a price. I imagine they believed their own scientists and technicians capable of isolating the necessary cultures and of developing their own processes. In this they failed. The same was true of streptomycin, which at the time of my visit in 1946 had not yet reached the importance of penicillin, but which was rapidly approaching it, particu-

larly because of its effect on tuberculosis. This private conference with the ministers gave me another opportunity to plead again for establishing close collaboration between the scientific groups in the two countries as soon as possible, for exchanging students and professors, and for establishing immediately a free exchange of scientific information. I was promised, here as well, that my plea would be presented before the presidium of ministers. I never heard anything further about it.

We spent as much time as we could spare from official visits to institutes, dinners, and conferences in the streets of the city and in many of its stores. Ridding ourselves of our guides, we mixed freely with the people, received civil answers to all our questions, and thus learned of their joys and woes.

In Moscow we found, in addition to Bobili's three brothers, several people from our native Priluka. They had managed to escape before the arrival of the Germans and had never returned home. From them, we learned of the sad fate of our town. At the outbreak of the war, the inhabitants there could not believe the current rumors of the brutality of the Germans, of their bestial behavior, especially toward the innocent Jews. They could not reconcile this with the prevalent concept of the Germans as a cultured nation. Most of the people remained, therefore, in the town, which represented no military value whatsoever. It was completely destroyed by the Germans and all its inhabitants were killed, as in thousands of other towns throughout the Ukraine. I was told that in Priluka only three inhabitants were now left: a girl who was hiding among the peasants; a fellow who had escaped to Central Asia and who had since returned from there; and the town fool, who had hidden in the woods and lived on herbs and occasional wild animals. All in all, it was not a very pleasant picture.

These old friends would come to our hotel room or we would assemble in Peisi's apartment. We talked late into the night. We saw passing before our eyes the horrors of the war, of total destruction, of human misery and of human bestiality, and of hatred that was implanted in the human mind. The Germans managed, in addition to outright destruction, to poison the minds of the Ukrainian peasants against the Jews, so that even those who returned to their homes found themselves in a most unhappy environment.

Moscow itself had suffered little from the ravages of the war. One of the professors took me to one of the windows of his laboratory and said: "We could see from here the approach of the German armies; they were only eight miles from this very spot." When I asked why Moscow had held out against the Germans, who by winning this city might have defeated the government of the Soviet Union and thus possibly have won the whole war, I received the following answer: "The Germans overextended their lines; they never expected that the Muscovites would put up such a terrific battle; the government remained in the city and took full responsibility for the war, and the people said, 'Moscow will never be given up,' and it was not!"

I paid several visits to the American Embassy, where I had some interesting conversations with members of the staff, exchanging impressions of the country and of the people. I kept them fully informed of my whereabouts. Whenever I could, both in further conversations with Dr. Vavilov and in the various conferences with other investigators, I tried to emphasize the importance of re-establishing as soon as possible the scientific exchanges between the two countries. The professors would have liked nothing better than to have my recommendation accepted; I found them in wholehearted agreement with me.

We also went to the theater, cinema, and museums. We saw several experimental farms, especially those devoted to the growth of wild roses and black currants as sources of much-needed vitamins. Most important to me were the visits to the various laboratories. The investigators were gradually settling back to their normal work after the war, although all the important institutions had been moved, before the approach of the Germans, to Asia or to the Urals, and it was hardly possible to do much scientific work under those circumstances. A scientist requires peace and concentration, scientific literature, and proper equipment.

After spending twenty days in Moscow, we left for Leningrad. The roadbed of the railroad line was in very bad shape, and the approach to the city was marked by many signs of the war. At the station, we were met by a large scientific delegation and were told that my lecture was scheduled within an hour. We hurried to the hotel, washed up, had something to eat, and arrived a bit late at the famous academy, which had been built by Peter the Great along the river

Neva. Here, I found a large audience waiting patiently for my address. I talked again about antibiotics.

The rest of the day and the day following we spent seeing the city, visiting the Pavlov Institute, and meeting various people. That once famous city of Peter was now becoming an almost forgotten, provincial town. The destruction in and around it was great. Particularly impressive was the destruction of the palaces. The Germans destroyed them methodically and systematically, as only they could. Not even a minute piece of mosaic was left which I could pick up and bring home as a souvenir. Even the parquet floors were burned out. And these palaces were of no military importance.

From Leningrad we proceeded to Helsinki, Finland. This was the slowest, most tedious train trip we had ever taken. The train stopped at little stations for hours at a time. Fortunately, there were with us several Norwegian diplomats, who were returning from signing a treaty in Moscow, and a few other western Europeans. We spent the time exchanging impressions of the Soviet Union. Our knowledge of German and French proved especially useful. Finally, we arrived late the next day in the Finnish capital. Advised of our coming by the American Embassy in Moscow, the American Chargé d'Affaires sent his official car to meet us and take us to the embassy, where we were to spend the night. The minister, Mr. Hamilton, a graduate of Princeton, and his wife proved to be most charming hosts. They were vitally interested in our Russian impressions and were disappointed when I told them that we would have to leave the next day for Stockholm and would thus have no opportunity to address a Finnish scientific audience. We were now very much pressed for time, since my lecture in Stockholm, arranged before my departure from the States, was due within two or three days.

Only one of my former Finnish students had heard of our arrival. He and his wife came to the station to see us off. He asked me whether I had any Finnish money. When I told him that I had had no chance to procure any (somebody commented during our brief stay in Finland: "Go into the streets and wave a dollar bill, and you will get for it all the Finn marks that you want."), he handed me a one-thousand-mark bill, and said: "You may need it to buy some wine to drink in the train when you eat your sandwiches." I exclaimed in surprise: "Why

should I take your money?" To which he replied rather confusedly: "Perhaps you will send us some coffee." That we were certainly glad to do upon our return.

There was a diner on the train. When we went there to get a bottle of beer, we were told that we could not buy beer without a meal. We spent about half of our Finnish marks for a meal that consisted of some thin soup, which was a welcome thirst-quencher, about a quarter of a pound of half-cooked carrots wrapped in half a fried egg, some potatoes, bread, and the thinnest and bitterest beer that I had ever tasted (apparently they did not have enough barley but plenty of hops).

The trip on the boat to Stockholm had its humorous touch. During the evening meal, which was paid for in dollars when we bought the tickets, I asked for some beer. The waitress inquired whether we wanted Finnish or Swedish beer. Having tasted the former earlier that day, we expressed definite preference for the latter. It was very good beer and we consumed it quite readily. When we offered our Finnish marks in payment, however, we were told that for Swedish beer we had to pay Swedish money. This we could not produce. I offered them American money, which was also refused because there was no exchange on the boat, and they were not allowed to receive dollars anyway. Thus, with Finnish money on a Finnish boat and with plenty of dollars, the best currency in the world, we could not pay for two bottles of beer. Our Norwegian friends saved the situation by paying for our beer. Later that night, when we tried to spend our remaining marks by offering to pay for some liquor, which this time we made sure was Finnish, we were told that we had the wrong marks altogether, since only "boatmarks" were acceptable on the boat. Here again, a friendly traveler, this time a Finnish gentleman who was returning home a few days later, took our marks and paid the waitress in "boatmarks." This exchange was quite illegal, since he was not allowed to bring in any Finnish currency. Such were the financial problems in many parts of Europe in 1946!

With a sigh of relief, we beheld the beautiful city of Stockholm the next morning. This was heightened by the fact that it was a glorious day and we were met at the pier by our old friend Prof. Barthel. He took us to a small hotel; later, he came in company with Prof.

H. von Euler and others to take us for lunch. Sweden did not suffer from the war and perhaps even benefited from it. It was the only cheerful spot that we were to encounter on this European trip, with the possible exception of Belgium.

The next evening, I delivered my lecture on antibiotics before a joint meeting of the microbiological and biochemical societies of Stockholm. By sheer coincidence, Dr. William Feldman of the Mayo Clinic was also in the city, where he gave an address on "Streptomycin in Experimental Tuberculosis" before the medical society on the following evening. Each lecture was preceded or followed by a dinner and reception.

At the first dinner, opposite me sat a lady, rather austere in appearance, who was introduced as Prof. Nanna Svartz. She spoke good English. Since my knowledge of medicine and of the medical world was still rather limited, her name did not signify very much to me. Without much preliminary, she addressed to me the following question: "Prof. Waksman, do you claim that streptomycin is a cure for tuberculosis?" I was quite unprepared for this, but somehow I had the feeling that I was being examined. My answer was quite frank: "What do I know about tuberculosis? Far be it from me to make any such claims. You probably know much more about this disease than I, and you must recognize that we are dealing here with a rather complicated condition, quite different from other diseases. A lot of time must elapse and there will have to be much experimentation before we know what part streptomycin can play in the control of this group of diseases. All that we demonstrated was that streptomycin is effective against various pathogenic strains of tuberculosis organisms in the test tube. If you would like to check these results, I will be glad to send you some streptomycin. Dr. Feldman and Dr. Hinshaw have found that it is quite effective in experimental tuberculosis. As regards clinical evaluation, the results so far obtained are too limited in scope to justify any broad conclusion. Certainly, they have not been carried far enough."

She seemed to be satisfied by my reply, since she smiled very graciously, and said: "So many great scientists have lost their reputations by claiming discoveries of cures of tuberculosis. Your own reputation is too valuable to us, and we should not like this to happen to you."

Please be careful never to speak of tuberculosis cures." I certainly appreciated this warning and assured her that I was not accustomed to making claims about something of which I knew very little; I sometimes even doubted some of the facts that I had myself established, resulting in repeated tests and experimentation.

Two months later, I received an airmail letter from Prof. von Euler, stating that Prof. Svartz had a rather interesting case of lymphatic tuberculosis and would like to try some streptomycin. At that time, we were receiving numerous requests from all over the world for this material to be used in clinical tuberculosis. We could not even attempt to fill a small portion of these requests. I, therefore, advised that possibly the Swedish Embassy could make such a request to the National Research Council in Washington, which was at that time handling the distribution of streptomycin through a committee with Dr. Keefer as chairman. This was done, and at Christmas time I received the following card from Prof. Svartz: "I obtained some streptomycin and treated my patient. He is apparently cured! Fantastic!" She herself now used the word "cured," whereas only four months earlier she had warned me strongly against its use.

Another story is concerned with Prof. Lilienstrand, a brilliant pharmacologist, who presided at Dr. Feldman's lecture. In discussing and summarizing this lecture, he commented as follows: "After a new discovery of gold, many people rush in order to collect the pebbles, and occasionally they find a gold nugget." The idea behind this statement was that after the discovery of penicillin, many people rushed to study antibiotics, and streptomycin was one such result. This was far from being the truth. But I could hardly respond to this discussion immediately. At the urgent request, however, of my friends in Stockholm, I wrote to Dr. Lilienstrand, assuring him that we had begun the study of antibiotics before the rediscovery of penicillin, that streptomycin had been isolated as a result of a rather difficult path that we followed and that had nothing to do with the discovery of penicillin. His simile was very brilliant, but the gold rush should be traced to Dubos' isolation of gramicidin; penicillin itself may have been one of the pebbles that he spoke about.

From Stockholm, we proceeded by plane to Copenhagen, where I delivered a lecture before the Danish Biological Society. Because

of overcrowded postwar conditions and because the International Food Commission was meeting in the city at that time, we had difficulty in finding hotel accommodations and were happy to accept the invitation of Prof. K. A. Jensen to stay in his home. We had a chance now to renew acquaintance with many of our former friends and several of my former students and collaborators.

Although Copenhagen did not suffer much devastation by the war, it was disfigured by many air-raid bunkers throughout the city, ranging from poor-looking proletarian shelters to de luxe affairs. Many of the stores had been emptied by the Germans. Although food was plentiful, there was a shortage of foreign currency and manufactured goods. Quite a contrast to Stockholm!

From Copenhagen, we proceeded by plane to Paris. My major reason for going there at this time was to arrange for publication of the collected works of Prof. Winogradsky. The venerable bacteriologist passed the whole war and postwar period at his home in the outskirts of Paris. He was now ninety years old, but his mind was still as acute as ever. With the outbreak of the war, he was deprived of the use of gas, which made it impossible for him to do experimental work. He lived alone with a niece, who took care of him; he was occasionally visited by one of his two daughters, who lived in Paris. He suffered privations, due to shortages of both food and clothing. He did not give up, however, but spent the war years in collecting all his scientific papers that had been published during more than half a century. Those papers that were written in languages other than French were translated into this language. He edited these papers, grouped them, and wrote an introduction and a summary to each group. He was now living in hope that his collected work would be published before he died. The Pasteur Institute, of which he had been a member for a quarter of a century, promised to help in this undertaking, but it was in difficult financial circumstances. Help had to come from America. A meeting was arranged in Paris with the publishers to decide what form this help might take. If America could only provide the necessary paper, the publication of the book could be undertaken. I promised to do what I could. It became evident later that we had to supply, in addition to the paper, also certain funds as an outright grant.

We also met the genial Dr. J. Trefouel, the new director of the Pasteur Institute, and his charming wife, both of whom have done some important work on the chemistry of the sulfonamides. We discussed various bacteriological problems, including the subject of antibiotics; the possible interchange of scholars between France and the United States and the fate of the Division of Soil Microbiology of the institute upon the death of Winogradsky were also considered.

We spent a day at the Winogradskys' home in Brie; it was a Sunday and his ninetieth birthday. At dinner, I drank a toast to him, greeting him in the name of all American bacteriologists, and I might have added, also European, since they were with me in spirit. I wished him happiness, many more years of life, and especially that he live long enough to see his great work published. We spent a few hours in the garden, listening to the old master tell of his war experiences. His whole family was there. His age was definitely showing and he walked with a cane. He inquired about new developments in bacteriology and was pleased at the progress that was being made in this field.

We had to limit our stay in Paris to only a few days, since the relatively high value of the franc made everything terrifically expensive. There was a great scarcity of hotel facilities, of taxis, and of good food. One would have to be very wealthy, or take advantage of black-market operations, or have an unlimited expense account, private or government, to get along. Since we could not fit into any of these categories, we proceeded rapidly to our next destination, Belgium, where I was invited to deliver three lectures, under the auspices of the Belgian-American Foundation.

Upon arrival at Brussels, we were met at the airport by a delegation consisting of M. Marteau, minister of public health, Prof. A. Gratia of the University of Liège, and Dr. M. Welch, one of my former students. The minister welcomed us to Belgium, presented a bouquet of flowers to Bobili, and informed us that he was placing a car at our disposal during our whole stay in Belgium. We went to the University House in Brussels, where we met a group of professors, had lunch there, and left immediately for Liège. It is here that I was to deliver my lectures and receive an honorary M.D. degree at a special university convocation. The last of the three lectures was to correspond

with the opening of the French-speaking Biochemical Congress, the first since the outbreak of the war. It was decided that my lectures should be translated into French and that I should deliver them in that language, any questions and discussion following the lectures to be carried out in English. This worked out satisfactorily, since most Belgians, unlike the French, know English fairly well. It was arranged that Welch undertake the translation of the lectures and look after their subsequent publication in book form.

This was the third time that I repeated this course of three lectures that year: the first in May at the University of Minnesota, the second in July in Moscow, and the third in Liège in October. Though the lectures were given in three languages, only the Russian and French translations were published. There was hardly any need for publishing the English originals, since I had already published many papers and addresses in which the material presented in these lectures was incorporated. The other single addresses that I delivered that year in various cities in America and in several European countries were condensations of this brief course on antibiotics.

Liège had suffered much from the war, some of the bridges and many of the buildings having been completely destroyed. After spending a few days there and meeting many of the scientific workers, we left for a week to see the rest of Belgium, notably Brussels and Bruges. These cities were hardly affected by the war. Everywhere we found stores packed with goods, markets full of produce, and restaurants that served the most delightful meals at rather reasonable prices. The people were gay, and there were carnivals and parades. What a contrast to France, where everybody appeared to be poor, where most of the people were shabby, where goods and food were highly expensive, and where everyone seemed sad and dispirited!

We returned to Liège in time to make a careful check of the translation of my lectures and to prepare them for delivery. Dr. Gratia proved to be a brilliant conversationalist and a charming host. He spent considerable time in entertaining us and in showing us about. He knew America well, having spent some years at the Rockefeller Institute. By a peculiar coincidence he was one of the first to study the production of antibiotics by actinomycetes in 1925, but at that time he did not fully appreciate the significance of this phenomenon. He

also observed, several years before Fleming, the injurious effect of a green mold upon the growth of bacteria, but here again he failed to recognize it as of great significance. No wonder that his teacher, Prof. Bordet, the venerable immunologist, said to him later: "My boy, the trouble with you is that you do not christen your babies."

At the opening of the congress, several of us received honorary degrees. One afternoon, Florey, Chain, and I assisted Gratia in a radio broadcast emphasizing the need for an antibiotic center in Belgium.

We were soon due to return home by plane and had to limit our attendance of the congress to the first days. We went by train to Amsterdam, where we arrived late in the afternoon. That same evening, we had a visit from our old friend Prof. Kluyver, accompanied by his wife, and by Dr. and Mrs. Waller of the alcohol distillery of Delft. They were also interested in the manufacture of antibiotics. We had not seen Prof. Kluyver since we parted seven years previously at the Microbiological Congress in New York. He had undergone a terrific change. He was thoroughly emaciated. The story of the hardships that he and his family experienced during the German occupation were very depressing. One could only feel a profound sympathy for this eminent scientist. His wife had to forage alone for food for the whole family; his son had to be hidden from the Germans so they would not carry him off to a labor battalion; he himself lost complete interest in the laboratory and stayed at home and brooded. Fortunately, he was now gradually recuperating. When I asked him to come to the United States, to learn what had been done in the field of microbiology during the war years, he answered: "Yes, perhaps later, when I will be able not only to take but also have something to give in return." Dr. Waller, a highly cultured European, treated us to a first-class meal and drinks; until far into the night we talked about antibiotics, microbiology, and the problems of the world at large.

The next morning we spent sight-seeing in Amsterdam, which was recuperating much less slowly than the Belgian cities, and left for home early that afternoon. We had dinner in Shannon, early breakfast in Gander, and late breakfast in New York. The trip was uneventful, except for minor engine trouble at Gander and for a threatened stop at Boston due to poor visibility.

We were met at the airfield by several members of the laboratory

and proceeded at once to New Brunswick. Here we learned that Byron had been discharged from the Army while we were abroad, and that he and his wife Joyce had gone South on a bicycle trip. They returned a few days later and, after a busy week, departed for Rochester, where Byron had accepted a fellowship at the Mayo Clinic.

The winter of 1946 and the spring of 1947 passed very rapidly. Streptomycin was now making history. Two great honors were soon conferred on me, that of Doctor of Science by Princeton University at the celebration of its Two-Hundredth Anniversary, and the Emil Christian Hansen Award in Microbiology by the Carlsberg Laboratory of Copenhagen, the first presentation of this medal to an American.

While sitting on the same platform with the President of the United States, the Governor of the State of New Jersey, the General of our Armies, Dr. Albert Einstein, and many other notables in the political, scientific, and literary worlds, and being cited by the president of Princeton University for the honorary degree because I had "led armies of microbes in battle against one another" (imagine the smile on General Dwight Eisenhower's face), I could not help but feel that microbiology had come into its own. The lowly microbes, the humble soil inhabitants and not merely the spectacular disease-producing organisms, were now receiving proper recognition.

Near me sat a tall gaunt gentleman, who was introduced as Mr. Eliot. As he received his honorary degree, President Dodds read his citation, and I recognized that this was the famous poet, T. S. Eliot, whose writings I used to read and admire when I was a student in college. When he returned to his seat, I congratulated him, commenting on the fact that I used to be an admirer of his. To this, he replied: "No wonder that you know more about me than I about you."

Incidentally, my efforts for Princeton in Moscow were only partly successful. A group of Russian scientists on their way to the United States for the purchase of scientific equipment were designated as delegates to the celebration. Upon my return, they had already departed.

The invitation to come to Denmark for the award and my interest in attending the International Microbiological Congress in Copenhagen, at which I was to serve as president of one of the sections, made it necessary for me to go to Europe again in 1947. I was also invited to deliver two papers before the International Congress of Chemistry

in London, just preceding the Microbiological Congress. We arranged, therefore, to go to England, Denmark, France, and Switzerland. This afforded additional opportunities to visit a number of laboratories and discuss various problems, especially the use of streptomycin in tuberculosis, which by now had become extremely important.

At the London congress, we met many old friends whom we had not seen since before the war, notably the Williamsons, who had entertained us in Egypt in 1938 on our return from the Holy Land, Sir John Russell, the veteran soil scientist, whom we had not seen since Cambridge in 1935, Drs. Florey and Chain of penicillin fame, and many others. Unfortunately, we had to interrupt our stay shortly after the chemical congress started, in order to fly to Copenhagen to the Microbiological Congress. We spent also two days in Nottingham, as guests of Boots Ltd., in company of their director, the genial Sir Jack Drummond, and his charming wife, both of whom were brutally murdered some years later on a camping tour to France.

The last day of our stay in London was a hectic one. The night before, we attended a reception at the Burlington House given to members of the congress by the Royal Society. Through some oversight, no tickets had been provided for us, in spite of the fact that the organizers of the congress, who had invited me to give the two lectures, knew of our coming. Fortunately, a kindly member of the Medical Research Council volunteered to give up his tickets, for which we were grateful, since it was at this affair that we met most of our old friends. When we were ready to leave the reception shortly before midnight, we found ourselves amidst a terrific downpour, and all the cabbies in London had completely disappeared. An old friend whom we had not seen in years turned up and offered to drive us in her small car to our hotel. Unfortunately, the car stalled in the middle of Piccadilly, and no amount of pushing could make it go. The rain was beginning to leak through the roof, and no help was in sight. Finally, a friendly truck driver came along and took us, in our thoroughly saturated evening clothes, to our hotel.

As we still had some packing to do, it was long after midnight before we went to bed. We had to get up quite early to be ready for my first lecture, at nine A.M. The second, an hour or so later, was held in a different building and before a different group, which involved

much rushing to and fro. We returned to our hotel at noon, picked up our baggage, and soon were at the airport, ready to leave for Copenhagen. We had a peaceful flight, arriving at our destination early in the afternoon. Here we were given excellent quarters; the food was superb, and the atmosphere far better than anyone would have expected.

The Microbiological Congress was a great success. More than a thousand scientists assembled from all parts of the world. Only the Soviet Union was not represented. The United States sent a delegation of more than fifty, from all parts of the country and representing various branches of the subject. I had been appointed by the U.S. State Department as one of the official delegates. The congress lasted a week, with sectional sessions in the morning and general sessions in the afternoon. Who could ever forget the genial president, Dr. Madсен, the friendly vice-president, Prof. Orla-Jensen, and numerous others!

Every afternoon or evening the meetings were followed by formal and informal dinners, receptions, banquets. Some of these one can never forget. One was in one of the university buildings and was given by Prof. K. A. Jensen in honor of the antibiotics group. Here, around a brilliantly lighted table, were seated many of those who had contributed to the development of antibiotics, including Fleming, Dubos, and others, both Europeans and Americans. When my American friends asked me to express in their name our appreciation to the hosts for this fine occasion, I could do no better than to tell the following story: That spring, in the process of trimming the rose bushes around our house, I had been hurt by a thorn. I paid no attention to this at the time. The next day, I happened to be in New York when I noticed that the spot on my finger had begun to swell. I entered a drugstore and asked the druggist to pull out the thorn, which he did. To protect the wound, I asked him to give me a package of adhesive bandages. He handed me a package, which had the word "tyrothricin" on it. This was the antibiotic developed by Dubos in 1939, and was now being incorporated in the pad. I looked at this name and, to find out how much the druggist knew about it, I asked him what it meant. He read it carefully, ty-ro-thri-cin, then said to me laconically: "That's some sort of a coal-tar derivative." When I ex-

pressed my surprise by saying: "Is that so?" he looked at me quite condescendingly and said: "This must be quite over your head!" The story brought down the house.

At the general meeting on the last day of the congress, I was given the Hansen Award, which consisted of a gold medal and five thousand Danish Kroner, to cover our travel expenses. In response to this award, I delivered the final lecture of the congress, "Antibiotics and Life." That evening Prof. Orla-Jensen gave a dinner at a very fine restaurant for a number of our old friends. Here were gathered many American and European microbiologists. The genial host was at his best. I never saw him again, since he died shortly thereafter. A great bacteriologist thus passed off the stage.

After the congress we remained for a few days longer in Copenhagen to participate in a world conference on biology, to which I had been appointed as one of the official American delegates.

We left for Paris by plane, where we arrived on a very hot and rather uncomfortable day in early August. It was now necessary to make final arrangements for the publication of Winogradsky's work. I offered to make every effort possible to obtain the necessary paper from America, which would facilitate publication. Upon my return home, I experienced great difficulty in obtaining the paper and permission to export it to France. Finally, with the help of the National Academy of Sciences, the necessary paper was obtained and also a grant for its purchase from the Rockefeller Foundation. Six months later, news was received from the publishers that they also required financial support. When the book was finally brought out two years later, it was a cause of general rejoicing among the microbiologists who valued so highly Winogradsky's work, which would otherwise have remained dispersed in many scientific journals throughout the world.

From Paris, we went by train to Switzerland. What a tremendous difference between the French and Swiss peoples! It was still the same train, but the change was almost shocking. On one side were half-hungry, dissatisfied people, recovering from the ravages of war, and almost ready for a revolution; on the other, a stolid, well-fed, happy people, bursting with riches. We spent two weeks at the Hotel Splendide, in Lugano. We took frequent walks, boat trips on the lake, train

and bus trips into the mountains. The food was excellent. We rested well after the two congresses and subsequent conferences. It was somewhat warm, occasionally even oppressive.

In Zurich we spent a few days. Here I was invited to visit Prof. Löffler, the head of the tuberculosis clinic of the State Hospital. I was received very cordially, and, after deciding upon the language which we were to use during our conversation, Prof. Löffler began to question me concerning the experience of the American clinicians with streptomycin. I was impatient to hear his own frank opinion concerning its effectiveness. Since I knew that here I would get an honest reply without mincing of words, without undue compliments, I asked him frankly what he thought of the potentialities of streptomycin in tuberculosis. His reply was: "Before I answer this question, you must come with me and visit my clinic; talk to my patients; the answer will be self-evident."

We proceeded to the immaculate clinic. Here we talked with about thirty patients who were being treated with streptomycin for the worst forms of tuberculosis. To some I talked in German, to others in French, to one in Italian, and to one even in Spanish (the last two through interpreters). In some cases I actually witnessed the administration of the drug to the patients, even directly into the brain. I was interested in observing the reactions of the patients, such as special pain caused by or following the injection, the effect of the treatment in causing dizziness or loss of hearing. Everywhere I was greeted by shining faces, grateful eyes, humble expressions of thankfulness and hope. We spent two hours in making the rounds of the hospital. The impression was profound. I had visited previously some of the clinics in America. But here was a highly selected group of cases, most of them difficult, which were being treated by experts who had devoted their lives to the study and treatment of tuberculosis. Here I would finally get a decisive answer.

When we returned to his office, Dr. Löffler said: "Now, I am ready to answer your question."

He began: "I have devoted my whole life, and I am now nearly sixty years old, to the study and treatment of tuberculosis. I have lived with this disease, I had lost my wife through it. Since the discovery of the tuberculosis organism by Robert Koch in 1881, we have

had numerous discoveries of cures for this disease, practically one every decade. First came the tuberculin cure proposed by Koch himself, then came the gold cure, the turtle bacillus, and many others." He continued to enumerate the various other cures and remedies that had been proposed for the treatment of tuberculosis, including finally the sulfonamides, sulfones, and penicillin. Then he added, "They all proved to be failures. Both the patients and the doctors became discouraged. Hopelessly, the conclusion was generally reached that while every known infectious disease may in time be cured by means of drugs, tuberculosis would not. In the case of this disease, the patient would have to depend entirely upon the time-honored bed rest and let the body take care of itself."

He paused for a few minutes and continued. "About a year ago, we began to get the first reports about the effectiveness of streptomycin. We then said to one another: 'Here comes another one of those cures!' And," he added, winking at me, "coming from America did not help it any! However, we decided to try it. We obtained a quantity of streptomycin through our embassy in Washington and treated a number of cases. You have just seen some of them in my clinic. I can assure you that half of them would not be alive today if not for streptomycin."

Dr. Löffler paused, then continued thoughtfully: "The best that I can tell you is that we are fully convinced that chemotherapy of tuberculosis may now be considered as a reality. Whether streptomycin will prove to be the last word or not, only the future will tell. We all hope, and I am sure you do as well, that it will not, and that better drugs will come. But it has already rendered a great service; it has fully established that it can be done. Not only is it already helping many people, but it is giving courage to all of us to continue the battle." This was certainly very encouraging to me. Although the same story was to be repeated to me, with variations, on numerous subsequent occasions, this was the first time that it had been put so bluntly. I was elated. I came home full of enthusiasm, full of hope for the future.

Since then I have visited many hospitals, talked to numerous physicians, including general practitioners, chest surgeons, and tuberculosis experts, both in the United States and in various countries

abroad. I have received homage from the many thousands of sufferers who have benefited from the use of streptomycin. There is now no question of its effectiveness. Continued use, especially when combined with other drugs or other methods of treatment, has made possible a better understanding of its more effective utilization. Its injurious reactions have gradually been reduced to a minimum. Its effectiveness has been increased to a maximum.

From Switzerland we went back to Paris, where I visited, among others, the hospital Salpêtrière. Under the guidance of Dr. Fouquet, chief of the tuberculosis division, I spent a morning chatting with some fifty patients who were receiving streptomycin for various forms of tuberculosis, mostly meningitis. There was always a bright light shining in their eyes. "Your visit gives them joy and courage," said Fouquet. "Not only are they getting better, but they are gaining hope for a subsequent recovery." I was particularly interested to determine whether any of them developed eighth-nerve reactions, which express themselves in dizziness or in a loss of hearing. This was definitely one of the serious limitations of streptomycin therapy. To my astonishment, I was told that only very few such instances had been recorded here, possibly because of the low doses used, as compared to the high doses employed in America. Incidentally, I was to discover later that not only in France, but also in other countries, was this the case, and the same explanation was always suggested.

As I was about to leave the Paris hospital, the director said to me: "We have a surprise for you." He led me into the court in the center of the hospital. Two small children came up to greet me: one a boy about five years old and the other a girl about seven. They were dressed in French national costumes and carried two huge bouquets of flowers. They curtsied and handed the bouquets to me. "Both of them were brought here in a state of coma last June, and now they are ready to go home," said Dr. Fouquet with much pride.

What could one add! This was ample compensation for all the efforts that I had made, all the worries that I experienced, and all the troubles that were to come. This unmitigated joy was, unfortunately, somewhat clouded a year later, when Dr. Fouquet wrote to me that the little boy had had a relapse and died. "But Colette is well," he

added. Thus streptomycin had its limitations. It was still an imperfect drug, especially when used alone.

We returned on the *Queen Elizabeth*. Since the French ports were not yet cleared of debris, we had to go to Southampton to board the boat. Although we made a reservation on a small overnight boat from Cherbourg, we discovered to our dismay that our cabin was already occupied by others. Three times as many people were crowded together as the boat had accommodations for. We had a miserable night crossing, with hardly a convenient place to lie down. On arrival at the British port, we went at once to a hotel for a bit of a rest before we went on the big liner.

The cabin adjoining ours was occupied by the Soviet Foreign Minister, Vishinsky, who was heading the Soviet delegation on their way to the United Nations Assembly in New York. Whenever we saw him, he was accompanied by guards. Although we would have been interested to meet him and engage him in conversation, the possibility that he might not quite appreciate our approach prevented us from doing it.

Thus we again returned from Europe richer in experience, with some added honors. This particular return may be compared with our return from Europe nearly a quarter of a century earlier. The 1924 return was the beginning of a scientific career. Then I went to Europe to learn, to saturate myself with the knowledge that the institutions of the old countries had to offer; this time, I went to Europe to teach, to instruct the audiences in a new science, one that had grown largely in America and had reached in a short time such tremendous proportions that all the world was anxious to benefit from it. In this, my laboratory had played an important part. To be sure, this was not the end of an era, since there was still much to be done; just as my earlier trip to Europe was not the beginning of an era, since so much had been done before. But these two visits were focal points which could be used to measure the progress of a science to which I had devoted my life, that of soil microbiology, and incidentally the progress of my own life as well.

Antibiotics Take the Stage

ONCE the efficacy of streptomycin in the treatment of numerous diseases of man and animals not responding to penicillin or to sulfa drugs had been established, feverish activity developed in the search for new antibiotics. This took place in my own laboratory, where more and more students and associates had been added, and especially in the numerous pharmaceutical organizations, first in this country and then throughout the world. The small laboratory could hardly keep pace with the giants of industry. Where I had a half-dozen assistants and associates and a dozen graduate students who came to be trained and incidentally utilized in connection with one or another problem, the pharmaceutical organizations employed hundreds of well-trained microbiologists, chemists, pharmacologists; they also had the advantage of large-scale production and co-ordinated efforts.

As a result, we were soon left behind in our productive research. Once we had pointed to the actinomycetes as a source of antibiotics, the field was thrown wide open. Whereas in 1939, when we started to work on antibiotics, hardly a dozen people in the whole world knew of or cared much about actinomycetes, now literally thousands of investigators were feverishly isolating these microbes from all soils of the world, studying their metabolic processes, isolating and testing various products for their antibacterial, antifungal, antiviral, and even antitumor properties. Soon marvelous new substances made their appearance, including chloramphenicol, the tetracyclines, our own neomycin, and a host of others. They came to fulfill the dream of the med-

ical profession to obtain tools for combating the numerous infectious diseases of mankind. They soon came to occupy an important place in the treatment of animal disease, in the feeding of animals, as certain preservatives, and in a variety of other phases of human endeavor.

Antibiotics became big business. The production in the United States alone soon reached hundreds of millions of dollars a year. Before long, they amounted to half of all the chemical products exported abroad; half of all prescriptions sold over the counter in the corner drugstore contained antibiotics. Where one antibiotic failed, another took its place. Soon combinations of two antibiotics, notably penicillin and streptomycin, or of one antibiotic and one chemical compound, as streptomycin and PAS, were being used in the treatment of tuberculosis.

These tremendous developments did not pass without heavy demand on my own time and energy. Although I could now give less and less time to actual laboratory work, I had to train and add people who could carry on the program. I was being called on more and more to deliver addresses and write papers and books on the new and rapidly expanding subject of antibiotics. In this respect, I could get only very little help from my associates and collaborators. Most of the work I had to do myself. The second edition of my book *Microbial Antagonisms* was prepared and sold out rapidly; a comprehensive volume on streptomycin had to be prepared and edited; a study of the rapidly expanding streptomycin literature had to be made; a new book on soil microbiology, perhaps my swan song in this field, had to be written. Because of the tremendous developments in the production of streptomycin and especially the fact that the university now controlled the domestic and some foreign patents, I had to carry a heavy share of the responsibility for co-ordinating all these activities, for organizing the practical aspects of the relationship between the university, as represented by the Research and Endowment Foundation, and manufacturing companies.

Since certain financial rewards were forthcoming from the last phase of my activity and since this resulted in great embarrassment and even unpleasantness to me, it may be of interest here to tell the story of this development in detail.

Pleasanter aspects. As told previously, the patents for the produc-

tion of streptomycin and other antibiotics developed in our laboratory had been reassigned by Merck & Co., in 1946, to the Rutgers Research and Endowment Foundation. The royalties accruing to the foundation from these patents proved to be far larger than anyone had anticipated and were more than sufficient to cover the normal needs of our department for continuation of the regular program of scientific research.

Were it not for our original arrangement, dating back to 1939, with Merck & Co. for close collaboration in the study of antibiotics, it is highly doubtful whether we would have ever applied for patents on antibiotics, which included, first, actinomycin, then streptothricin and streptomycin, and later others. It is also doubtful whether, without such connection, our work would have ever taken such a practical turn into the field of human and animal chemotherapy. Now, however, since such large funds had become available to the university (and it was tacitly assumed that funds derived from processes developed in the Department of Microbiology should be used for research in that department), several questions presented themselves. One was what course of research our department should pursue further. Should we return, now that we had made an important contribution to general and medical microbiology, to our original problems of soil microbiology? Should we enlarge the work of the department to embrace a broad program of general microbiology? Should we branch out into the field of chemotherapy, or concentrate on a field of research where our work had already found such wide practical application? These questions were largely motivated by the excess of accumulating funds from the streptomycin royalties.

The question had even come up whether these excess funds might not go to the university as a whole for its various needs. The Experiment Station, which had faithfully supported my work for more than three decades without questioning its theoretical importance or potential practical applications, had certain rights to the use of such funds. The question was also raised whether I was entitled to some supplementary financial consideration in addition to my regular salary. This was the first time that either I or anyone else gave this matter any consideration.

The problem of using the excess funds was solved without any great difficulty. It came about in quite an unexpected manner. "What

sort of farmer are you?" asked Mr. Russell Watson, the legal adviser of the Rutgers Research Foundation. "Your work now belongs in a university, and not in an agricultural institution. It has long outgrown its original narrow boundaries!" Thus an idea was born. I had long dreamed of properly equipped laboratories, where investigators and students interested in the life of the microbes, the smallest of living things, could come together and work on fundamental problems. I had long dreamed and talked, whenever I had an opportunity, of microbiology's finally coming into its own and ceasing to be a science that had always been at the tail end of other sciences. I recognized at once the potentialities latent in Mr. Watson's question and suggested that we organize an institute which should be devoted to the subject of microbiology and which would be an integral part of the university.

The proposal to establish an Institute of Microbiology at the university was enthusiastically received both by the university authorities and by the outside world. The president of the university immediately appointed a committee of trustees to give this question the most careful consideration. At its next annual meeting, the committee recommended to the board of trustees the establishment of the institute, as an independent entity within the university. An official announcement to that effect was made in the office of the president of the university before representatives of the press, on May 5, 1949. In addressing this gathering, I said:

Evidence is accumulating that we have but scratched the surface of the potentialities of microbiology, not only in medicine, but in agriculture, industry, and all other fields of human endeavor. In addition to serving as a center for research and for the training of investigators, the institute will perform three other very important functions: first, it will serve as a gathering place—a Mecca—for microbiologists from all over the world; a place to which they can turn for the free interchange of ideas which is the lifeblood of scientific progress. Second, it will co-operate with governmental agencies, such as the Public Health Service and other Federal units, which have important interests in the development of basic knowledge in microbiology. Third, it will collaborate with the many industrial and medical organizations which have equally vital stakes in microbiology and which have been seriously handicapped in the past by the shortage of research facilities and of facilities for the training of qualified men to apply the fruits of fundamental research.

The president of the university was very emphatic in presenting his own point of view:

No announcement I have made at Rutgers has given me more pleasure than to report the decision of Dr. Waksman to devote the proceeds of his great discovery to further research and the actions of the Rutgers Research and Endowment Foundation and the board of trustees in establishing an Institute of Microbiology. Establishment of the new Institute of Microbiology as a major branch of the state university will permit a full-scale exploration of the potentialities of these microorganisms as instruments for human betterment. Equally important with the fundamental and applied research will be instruction at the graduate levels so that greater numbers of future scientists can benefit from the long years of study which led Dr. Waksman to the discovery of streptomycin.

The public and the press met the announcement of the institute with universal acclaim. Laudatory editorials were devoted to it in newspapers and magazines, and numerous letters expressing sincere appreciation and offering hearty congratulations were received from all parts of the world.

Honors and awards poured in uninterruptedly. The Advertising Club of the State of New Jersey elected me as the leading citizen of the state. A testimonial dinner was given by the service clubs of the city of New Brunswick; the Veterans of Foreign Wars of New Jersey gave me their annual medal; the New Jersey Agricultural Society gave me a gold medallion; the New Jersey Tuberculosis Association gave me their annual award; the city of Philadelphia gave me the John Scott Award; the American Pharmaceutical Manufacturers Association gave me their award; the American Public Health Association gave me the Lasker Award; the American Academy of Arts and Sciences gave me the Armory Award; there were many others.

In accepting the award from the Advertising Club, on May 6, 1949, I said:

I can only say that to one who has spent most of his active life in this state, no award could be more welcome. The State of New Jersey has given me my basic scientific education and all the opportunities that any man, striving for a scientific career, could hope for. It is here that I first came in close contact with the soil to which I was to

devote my whole life. The soil on this farm had magic to it. When I dug my hands into it and allowed it to sift through my fingers, I marveled at its richness—its fertility. What caused this fertility? I wondered. What chemical and biological processes went on there? What numerous forms of microscopic life did this earth contain?

With the exception of two years spent on the other side of this continent, in another great state, namely California, I returned to New Jersey and have stayed here ever since, both as a resident of this state and also as a member of the institution that gave me the first foothold in my scientific career. I am only one of the many who have come to this country—the many to whom Woodrow Wilson referred when he said, "The blood of all the world is in our veins, the old affections and the old and sacred traditions of people everywhere." I can only add that if in a tiny measure I have helped make America what it is today, a great country, a good country to build and live in, I am deeply grateful.

Thus it appeared that my Odyssey was coming to an end. There was only one hope, that I might live long enough to bring the institute into being as a great center for microbiology, an institute which would serve as a monument to all those indefatigable scientific spirits at Rutgers who had preceded me, notably Dr. Cook, Dr. Halsted, Dr. Voorhees, and Dr. Lipman, and to all those of my students and associates who were to follow me, and who were to carry on further, in the spirit of truth, devotion to research and the improvement of the well being of mankind.

Some sad developments. The future that appeared so bright and so full of promise in 1949 was headed for a severe blow. This came from quite an unexpected quarter. It came from one of my own former students, one in whom I had great confidence as a promising future scientist, one whose name I had placed side by side with mine on numerous scientific papers, one who I hoped would carry on in the same tradition and spirit of research as I have done myself, as Dr. Lipman did before me, and as some of my former students were already doing.

I always prided myself on the good relations and mutual confidence between myself and my students, associates, and visiting investigators. Out of some one hundred and fifty men and women who have worked under me or with me during the thirty-five or more years of my scientific activities, many were able to pick up where I left

off and carry on further, in the spirit of true scientific research. I have always made every possible effort to give to all my graduate students and assistants as much scientific credit as was at all possible for their work, in an attempt to encourage them to devote themselves to science as a career. This is by no means a universal practice in research laboratories. Now, at the height of my own scientific activities, I was to experience trouble because of this generous policy.

Early in March, 1950, I was served a summons from a judge in New Jersey to appear in court to give evidence on a complaint filed against me and the Rutgers Research and Endowment Foundation by one of my former students. The complaint alleged that, by virtue of my power, position, and influence in the field of microbiology and in the world of science, as contrasted to the student's youth and inexperience, I would see to it that he would be refused employment in the scientific and professional field for which he was trained, that no reputable institution would employ him, if he should embarrass me by refusing to execute an assignment to the Rutgers Foundation of a patent on which his name appeared; that if he should refuse to exercise such assignment, I would cause his name to be withdrawn from the patent application. The complaint contained these and other claims and included demands for a share of the streptomycin royalties and for an order restraining me from representing myself as the sole discoverer of streptomycin. What a terrific shock it was to me!

Mr. Watson, the lawyer for the Rutgers Foundation, immediately issued the following statement to the press:

Streptomycin was a result of nearly thirty years of continuous, systematized study conducted by Dr. Waksman which led to the development of antibiotics, among which is streptomycin. This student was one of about twenty technical assistants and graduate students who participated in this extensive research from time to time under Dr. Waksman's direction. The student's work prior to the discovery of streptomycin covered a period of only about three months and was performed in the capacity of a carefully supervised laboratory assistant. He was a small cog in a large wheel, the revolutions of which embraced the isolation of the culture which produced streptomycin, the development of media for its production, methods for its isolation from the medium, determination of its potency in the test tube and in experi-

mental animals, clinical evaluation, manufacture on a large scale, and the development of a comprehensive program for the investigation of its therapeutic potentialities. It was Dr. Waksman's uniform practice to give recognition to his assistants by including them as coauthors of the first published papers and cosigners with him of patent applications. Dr. Waksman so recognized his various assistants in the case of actinomycin, streptothricin and streptomycin, and in the case of grisein and neomycin, two discoveries which have followed streptomycin.

Certain facts must be clearly understood before the significance of this development can be fully appreciated.

1. My work on the actinomycetes dated back to 1914. I began, in 1939, to examine these organisms for their ability to produce antibiotics. In this, I was assisted by a number of graduate students and laboratory technicians. Our first antibiotic, actinomycin, was isolated in 1940. It was too toxic. In 1942, we isolated streptothricin, which, except for a certain limited toxicity, possessed all the properties of a highly desirable chemotherapeutic agent. We were now searching for other agents similar to streptothricin. The student in question returned from the Army on June 30, 1943. On August 23, 1943, we isolated a culture of an organism, long known to me, *Streptomyces griseus*. This culture was found, by the methods developed for the production and isolation of streptothricin, to produce a similar antibiotic, which we designated streptomycin, a name coined in the laboratory the previous January. Further tests carried out in our laboratory and in the laboratories of Merck & Co. proved it to be a highly desirable substance with potential chemotherapeutic properties.

2. The application for the streptomycin patent was made under a contract arrangement between Merck & Co. and Rutgers University, dating back to 1938 and 1939. In signing, for my laboratory, the contract with the company, I automatically gave up all patentable rights of my own and those of my collaborators, since nowhere in these contracts was there any mention of special compensation to be given any of my collaborators or me from practical developments resulting from our work. Any royalties resulting from practical discoveries automatically became the property of the university.

When Merck & Co. filed a patent application covering the work done by me and my collaborators, they usually placed on it my name

and that of the particular assistant or student who helped me in the specific problem. This was true of various fermentation patents, of the actinomycin and streptothricin patents, each time the name of one of my students being added to mine. It may be interesting to note that United States patent law requires that a patent application be executed by all co-inventors. It is sometimes difficult to draw the line precisely between assistance and invention. In such cases, it is prudent that the patent application be executed by both the inventor and the assistant to avoid any possible legal defect, and that the assistant execute an assignment of his rights, if any, in the invention. Such was the case in the streptomycin patent to which was added the name of the former student.

3. Certain foreign applications had been filed, on the basis of the American patent, by the Rutgers Research and Endowment Foundation in my name and that of my former student. Subsequently, he refused to make assignments of some of these applications to the foundation. It would have been necessary to take court action to compel him to do so.

4. Soon after the Rutgers Research and Endowment Foundation was organized and took over the patents from Merck & Co. in 1946, the trustees suggested that I be given twenty per cent compensation for my efforts in the development of streptomycin, not so much because of the discovery of it, but for organizing the license arrangements with the various companies and for carrying above my normal duties all the burden of the practical developments of the production and utilization of streptomycin. This included preliminary agreements with industrial organizations for the manufacture of streptomycin, visits to various plants, and testing various products. Simultaneously, I had to sign a contract with the foundation that I would not leave the university for practically the rest of my scientific life. I agreed to accept such special compensation, not, however, in the form of an outright salary, but as twenty per cent of the accruing royalties. I felt that should such funds come into my possession, I could use them to advantage in advancing the field of microbiology, by making grants to various institutions, establishing fellowships, and encouraging publication of results. When, however, the sums of money coming from the royalties proved to be much greater than anyone could have anticipated, I demanded

that my share be cut in half, namely to ten per cent, the other half to go to the Institute of Microbiology which was already in the planning stage. Later I assigned a half of my remaining ten per cent to a Foundation for Microbiology. This foundation was to use its funds to promote microbiology throughout the world.

5. The remaining ninety per cent of all the funds accruing to the Rutgers Foundation from streptomycin and other royalties were to be devoted to the establishment of the institute.

The days that followed the complaint were the most unpleasant of my life. I was in almost constant consultation with the attorneys. On at least two occasions I was required to appear at pre-trial hearings and was subjected to seemingly endless questioning by the plaintiff's attorneys. I felt that I would probably spend the rest of my life in courts, helping to prepare briefs, educating and answering lawyers. My whole department was completely upset by a continual series of rumors. It now appeared as if my scientific work was to come to a complete standstill, that the institute might never be built, and that all my dreams would be shattered. The publicity given by the newspapers to the streptomycin millions left an impression that I gave them to the university, but no doubt must have kept some of the millions for myself.

When I was faced with the mass of evidence that had been accumulated by our lawyers, the decision as to what to do further had to be made. To proceed with the case, or to settle it as amicably as possible? One's pride, one's feeling for justice and for one's friends, all demanded that the first course be pursued. On the other hand, some of the factors listed previously, especially the possible delay in the building of the institute which would culminate my lifetime work and devotion to science, and finally, the prospect of having to face daily attacks in the press during the days in court—all made me decide that, after all, the second course might be the wiser.

I endured nearly a year of this nightmare existence. I had the consolation that nearly all of my students and assistants, both former and present, flocked to my support. Many of my friends, however, urged me to settle the suit, to get on with my research and speed the fulfillment of my dream, the construction of the new institute. Some of this well-intentioned advice, I had the feeling, took the form of pressure.

I also felt a moral obligation to my associates, to my university, and to all others who had supported my work throughout many years, who would have been involved in the drawn-out and unpleasant legal procedures. I reached the conclusion, not without great reluctance, that the situation made a speedy disposition of the lawsuit essential. The decision to settle the suit was largely mine. It took me many months to come to that decision. Finally, the counsel of those who urged the second course prevailed, and, with a pain in my heart, I agreed to it.

In the court-approved stipulations, the charges that I had used fraud and duress were withdrawn. The former student was legally and scientifically recognized as the coinventor. Eighty per cent of the royalties remained with the foundation for the construction of the institute and the furtherance of research in microbiology and related fields. The student was paid \$125,000 for the assignment of all foreign patent rights and three per cent of the royalties.

I recommended that the original half of my share in the royalties, namely the ten per cent that I had returned to the Rutgers Research and Endowment Foundation, should be distributed among all twenty-six of my former students and assistants who had helped in the development of the antibiotics program which led to streptomycin. After all, there would have been no streptomycin had it not been preceded by streptothricin; there would have been no streptothricin were it not for actinomycin. Furthermore, if a student or assistant who streaks out a bacterial plate, or inoculates a flask, or makes a chemical determination is entitled to scientific consideration, the assistant who washes the dishes, or makes up the medium, or prepares the chemicals has also made a contribution toward the solution of a scientific problem, even if his name does not appear in the scientific report. A great scientific contribution is like a beautiful mosaic made up of many stones, or like a marvelous symphony to which many musicians contribute their share, but no one of which constitutes the mosaic or the symphony itself.

It may be of interest to record here a statement submitted on November 1 and 2, 1945, in connection with the hearings before a subcommittee of the Committee on Military Affairs of the United States Senate, of the Seventy-ninth Congress:

The discovery of streptomycin is another illustration of the importance of planning biological research in the solution of a given scientific problem. When a comprehensive project of a systematic study of the production of antibiotic substances by different groups of microorganisms was undertaken at the New Jersey station, various approaches to the problem were at first examined. It did not take long to discover that microorganisms capable of producing antibiotics are very abundant in the soils and in composts, that such microorganisms produce more than one type of antibiotic and that these substances vary greatly in chemical nature, selective antibacterial activities, toxicity to animals, and activity in the animal body. It also became soon apparent that such a study would involve detailed surveys of large numbers of organisms and that long, tedious research would be involved. Actually, within a period of five years, some five or six thousand organisms were examined in these laboratories. These included molds, bacteria, and actinomycetes. With limited funds and still more limited personnel, this was not a very easy problem. Fortunately, one of the foundations and one of the industrial organizations came to the support of this project. The subject attracted graduate students to the university, intent upon making this problem a subject for their doctor's thesis. A dozen or more collaborators were thus engaged in this comprehensive investigation.

As I look back upon the year 1950, I consider it the darkest one in my whole life. Here I was at the height of my scientific career, acclaimed universally for having given to the world what promised to be the solution of the tuberculosis problem, what might lead to the final eradication of the "white plague" of mankind. Here I was receiving honors from leading universities and scientific bodies throughout the world, serving on international committees, as honorary president of important international congresses—about to be dragged into court and suffer indignities, at the instigation of one of my own students, whom I had educated, to whom I had pointed a way into the field of science, and whom I had believed capable of becoming one of the brilliant stars in the firmament of my group of students, the school of young microbiologists whom I had trained.

Whether, in settling the above case as I did, I made a serious blunder, the future will tell.

In Spain and Elsewhere, 1950

WHILE the legal wrangles of the case were taking place and in order to have some peace of mind, as well as to fulfill a number of standing obligations and invitations, I decided to undertake another journey abroad in 1950. This proved to be one of the most exhausting trips that we had ever undertaken. It led to a number of subsequent journeys.

Among the invitations that came that year, there were several to attend international congresses. These ranged from the Botanical Congress in Stockholm and the Microbiological Congress in Rio to a variety of different medical congresses in Paris, Zurich, and Rome. Besides, there were several invitations from some highly specialized conferences, and a number of invitations to medical weeks in Toulouse, Verona, and elsewhere. I could go only to the Botanical Congress, of which I was elected as one of the honorary presidents.

There was another, even more important reason for going to Europe that year. This was the invitation of the World Health Organization in Geneva to attend a meeting of a newly organized Committee on Antibiotics, to plan a program of research, education, and application of antibiotics in the various backward countries of the world. The Antibiotics Committee was to meet early in April and the Congress in mid-July. To fill in the intervening two months or so, I decided to accept a long-standing invitation of the Scientific Council of Spain to deliver several lectures in that country, as well as of the University of Madrid to receive an honorary doctor's degree. This would also

allow me sufficient time to go to Paris to organize the foundation that was to receive the royalties from streptomycin manufacture in France; these funds were to be used for furthering scientific research in microbiology and in supporting exchange fellows between France and Rutgers. Should there still be a few weeks left, we could spend them in Rome, which we had not visited for more than a quarter of a century, and in Toulouse, where I had been invited to deliver an address.

The Antibiotics Committee that met in Geneva were all of one mind. It was recommended that the only way to help backward countries to take advantage of the rapidly growing field of antibiotics and their applications was not to build factories in each country, but to train teams of scientists and study the practical utilization of new discoveries.

Immediately afterward, we left Geneva by night train directly for Spain. We arrived at the border early in the middle of April in the morning, in a drizzling rain. We were met in a car by Dr. F. Bustinza, who was to be our guide and constant companion throughout our stay in that country, and were driven to Barcelona. The weather being bad and the roads abominable, the journey was not too pleasant.

On arrival in the Catalan capital, we were given a grand reception at the City Hall, where I had to make a radio address. Then we were taken to a magnificent restaurant on a hill overlooking the city and treated to a marvelous *paella valenciana*, a form of the famous "spanish rice." As it was now after five o'clock and we had had no food since early morning, we did full justice to the dish, despite the mob of people, mostly doctors and pharmacists of the city, surrounding us. We had to take a night train to Madrid and had little time to enjoy the hospitality shown to us. The mayor of the city and members of local, state, and medical circles saw us off at the railroad station. Bobili was showered with flowers.

We arrived in Madrid on a bright Sunday morning and were met by a scientific delegation from some of the institutions of learning. After a hasty breakfast and some unpacking, we went to see the Escorial. The day was perfect. We saw the university city, where heavy fighting had taken place during the Civil War. We stopped on the way at a charming restaurant overlooking the Guadarrama Mountains

for our first typically Spanish lunch. The *paella* in Barcelona the night before and this lunch convinced us that Spain had much to offer in satisfying our gastronomic tastes.

Arrangements had been made to have two priests guide us through the Escorial. One was a young ascetic English-speaking individual, somber and monarchistic in his leanings. The other was a very merry middle-aged fellow, who spoke French and appeared to have just stepped out of the pages of Rabelais; one could picture him leading another revolution against Franco, for whom he had little use.

We were duly impressed by what we saw inside the building, but even more so by the world outside the dead walls, the grounds, the scenery, and the priest in a meadow blessing a cow while the owner was milking her.

The next day proved to be quite exciting. The Council of Scientific Investigations of Spain was completing a week of celebrations of its tenth anniversary. That morning a special final session was held, with Franco presiding. We were invited to attend and upon arrival were given seats in the front row, so that we had a good view of the dictator. Many of the participants were formally dressed, academic and ecclesiastical costumes predominating. After several very lengthy and rather tedious speeches, Franco addressed the audience on the importance of science. He particularly emphasized "Christian science," in modern society. He was presented with numerous books, beautifully bound and representing the work supported by the council. Many of those who came up to present their work were clerics. A peculiar brand of science indeed! Sitting near us was the representative of a famous foreign power, with whom we exchanged some caustic comments regarding the proceedings.

My own address was to come later in the week, a week spent in a whirlwind of entertainment. There were several dinners, given by the Medical Academy, the Pharmaceutical Academy, the Ministry of Education, and others. The most important event took place later in the week, where I was presented by the university with the degree of Doctor *honoris causa*. We were met by a crowd of applauding people. The large university auditorium was packed. The faculty was garbed in academic attire. The professor of bacteriology made an oration. The rector was all that he should be, very dignified and charming. In pre-

senting me with the degree, he delivered a very fine address not lacking in humor. He embraced me. Numerous photographers and motion-picture operators were busy all the time.

Such was the beginning of our fame in Spain that spread like wildfire first through the city and later everywhere we went. We could not pass in the street without being recognized. Autograph hunters were constantly after us. Newspapers and magazines carried our pictures constantly. All sorts of societies began to approach us to attend their meetings or be their guests, to make one address or another.

My lecture at the council, given in English (my subsequent lectures in the other cities were in Spanish), was well received. One curious episode occurred. Just as I was entering the auditorium, a lady dressed entirely in black approached me and handed me a package. When I opened it after the lecture, I found a book of poetry—*Luces de una Alma* (*The Light of a Soul*) by Justa de Matas, a prominent poet, the story of which I recorded previously. There were other events similarly touching.

We left by train for southern Spain. While the week in Madrid was hectic, the days spent in Seville and in Granada were beyond description. Literally, we had not a minute to ourselves after we arrived at the Andalusian capital. For four days, we were dragged about from place to place. Our hosts were the members of the Pharmaceutical Society, before which I delivered my first address in Spanish. Although my pronunciation left much to be desired, I was understood much better than had I given it in English or French. I was presented with a diploma, a beautifully engraved silver plate mounted on marble, and a number of other gifts.

Apparently the whole executive committee of this society took a vacation to be with us at all times. There were numerous visits to museums, to churches new and old, to art galleries, and to other places of interest.

The high point was a dinner at the Alcazar given by the Provincial Governor, where we were entertained by a group of *flamenco* dancers. This was followed by a night walk through the streets of the city, especially the old Jewish quarter. All of this was fantastic, when one considers that we were followed by literally hundreds of people.

Early the next morning, we left in a car placed at our disposal by

the Governor of Granada. Special police were waiting for us at the outskirts of the city, and as soon as our car arrived, they cleared the way for us. We were led directly to the City Hall. In view of our exhaustion from lack of food and general ill health, we insisted upon being taken to our hotel, the Alhambra, where we were assigned a marvelous suite and allowed to rest a few minutes. The rector of the university and various members of the faculty were waiting for us at the Generalife (summer palace of the Arab caliphs).

To say that the Generalife was beautiful in moonlight would be but a poor description of the panorama that unfolded before my eyes. The marvelous moorish gardens, the fountains, the constantly murmuring springs, the singing of the nightingales, moonlight falling on the cypresses, the view of the city and the Alhambra—all this was truly a page from the *Arabian Nights*. I was so bewildered, nay bewitched, that I could hardly give ear to the words of the assembled members of the university and of the medical world. I hardly remember the taste of the food, although I was hungry, not having eaten much since early morning. Next day, we made a round of visits. Late in the afternoon, I gave my address before the Medical Society, and was presented with a medal, a diploma, and a variety of gifts. This was followed by an official dinner at the Arab University. Then we were taken to a gypsy cave to witness a marvelous performance of gypsy dances and music that lasted well into the night. The next and last day in Granada, we made a series of excursions through the new and old sections of the city, guided by Dr. de Lucena, a brilliant Arab scholar who had much to tell us of the history of this famous city.

That evening, we boarded the only sleeper on the night train for Madrid. We were very tired and went to sleep at once. In the middle of the night, our car broke down, and we had to dress, change over to a coach, and sit up the rest of the way in a cold and cheerless atmosphere. A bottle of fine muscatel presented to us by the abbess of a monastery in Seville kept up our spirits and made us feel warm. We were back in Madrid Sunday morning and took a good rest that day. The next day, we started for the Basque country by car. We had a fine view of Old Castille and we reached Burgos in time for lunch. Waiting for us were the ever-present photographers, the mayor, and several other dignitaries. The lunch proved to be a feast, and a good

one too. After a proper fiesta in the gardens of the host, we went to see the famous cathedral, then left for San Sebastián.

We arrived at the hotel well after dark, were allowed only a few minutes' rest, and then were rushed off to a private club, where we were treated to a fine dinner and a performance of Basque dancers and singers.

The next morning, we were taken, by car and on foot, for a sight-seeing tour around the city and, after lunch, were driven to Bilbao. On the way, we stopped in the town of Eber for a reception at the City Hall. We were greeted by cannon fire and a mass of applauding humanity, and finally were allowed to enter a sumptuous room, where the city dignitaries presented us with still more gifts. We had some refreshments and watched from the windows performances of Basque dances. These people try to present their cultural autonomous tendencies through their songs and dances. When the sports club of the region presented me with a letter announcing that they were going to name a prominent mountain after me, all I could think of was: Certainly the lowly actinomycetes have grown to mountain size. Finally, late that night, we came to Bilbao. Fortunately, the reception committee, possibly on advice of their mayor, a man with a fine sense of humor, left us in peace so that we could rest for what was to come the next day.

The same round of receptions and dinners followed, with interesting variations: a luncheon in the city home for convalescents, some miles from Bilbao, with a beautiful outlook upon the sea, marvelous food, especially shellfish, in company of the charming mayor, Dr. J. Zuazagoitia, a scholar and poet. We spent a day on this trip, visiting on our return the old Basque shrine, the oak tree at Guernica, with the museum of Basque antiquities. One of the cars in the group kept breaking down, evidence of the poor rolling stock now prevalent in Spain. The reception at the City Hall was another affair to be remembered. There were several representatives of the American consulate, some outstanding social and civic leaders, music, and some fine decorations.

After the two days in Bilbao, we returned by car to Madrid, where we made final arrangements for departure from Spain. The plane that was to take us to Rome was due from New York at one A.M.,

but it actually arrived at four A.M., and we had to sit up most of the night at the airline station. Fortunately, we had good seats in the plane and, after an uneventful flight, landed at the Ciampino Airport in Rome.

Thus ended our Spanish days. We had been tired when we arrived at Barcelona, after bumping along for a good part of a day in an ancient car. We were even more tired when we left Spain three weeks later. The friendliness, hospitality, and love of music of the people, the lack of hurry to accomplish something, and a certain moodiness of the intellectuals, all reminded us of the Russians under the old Czarist regime. Even its revolution was like the Russian, except for one difference. The middle class of Russia was decimated as a result of the revolution, while the military was so exhausted by the three wars since the beginning of the century that it gladly went along with Kerenski and hardly put up a fight when Lenin took over. In Spain, however, the middle class was strong and the military undefeated, except on the foreign battle fronts—all of which played into Franco's hands.

In the few hours it took to go from Madrid to Rome, we were transported to a new world, with new hopes and new aspirations. Having rid themselves of Mussolini and his hordes, the Italians were now expressing their new-found freedom in city noises. The infernal racket of scooters and cars on Roman streets seemed to be an expression of the people released from Fascist bondage.

Because this was Holy Year, I had made arrangements for hotel accommodations before we left the States. Upon our arrival at the Hotel Flora, we were told that no room would be available until late that afternoon. We were extremely tired after our sleepless night and would have to stay unwashed, in a drafty lobby, for no one knew how many hours. Our protests and those of the plane representatives were of little avail with the crafty fellow at the desk. Finally, a man arrived from the Italian company that had made the reservation for us. Lo, a room immediately materialized. It was a noisy room. Still it was a place where we could wash up, have some food, and try to get some rest.

The next day Dr. Ernest Chain came to see us and took us in his car for a sight-seeing tour of the city. Chain had been in Rome for

nearly a year and had come to love the city, every corner of which he knew. He finally persuaded us to give up our plan of going to Naples and Capri and, instead, spend two weeks in Rome. An invitation came from Dr. Marrota, director of the institute, to join him at lunch in a beautiful restaurant in the Pincio, overlooking the city. He asked me whether I would be willing to deliver an address at the institute during our stay in Rome. It would have to be given, if not in Italian, at least in French. Since I had just received from Toulouse the French translation of my address to be given there, this might afford me an opportunity to read it over, and I gladly accepted the invitation.

The two weeks spent in Rome passed rapidly. They included several receptions, dinners, scientific and tourist visits, and general loafing, whenever a few minutes were to be spared. Dr. L. Cherubini, a tuberculosis specialist, who had visited me previously at New Brunswick, suggested that we have an audience with the Pope as well as attend a sanctification ceremony. He arranged it himself, since he was an old friend of the Pope's and had apparently a close connection with the Papal hierarchy. Both affairs proved highly interesting.

The Papal court in the Vatican was magnificent, and Pope Pius XI proved a charming person. He seemed to have been previously informed as to what I was doing. He said quite emphatically: "At a time when so many people spend their time and energy to destroy others, it is good to know that a few are working for improving the lot of man and for curing disease conditions that afflict mankind." We were properly blessed, given silver medals, and allowed to depart in peace.

The other ceremony was much more exhausting. We had to get up early in the morning, have a hasty breakfast, and depart, in a car provided for us by our friend, to St. Peter's. There, we were whisked through back streets, into the immense church. We could see every move of the Pope and of the dignitaries sitting in the front aisles. It was a very impressive ceremony. The chanting resembled a good deal the chanting in the synagogue to which I used to listen in my youth. We had to sit on a hard bench for more than four hours, with black-robed nuns all around us. When we finally managed to escape the crowds, the fresh air and the Roman sunshine felt like elixir.

We spent one evening with Prof. Caronia and his wife. He was

in charge of a large clinic on tubercular meningitis, and his wife supervised a convalescent home for tubercular patients at Ostia. They both were without words to express their admiration for streptomycin. Before the advent of streptomycin a total of fifty cases of tubercular meningitis had been cured spontaneously in all past history throughout the world. Since 1947 Dr. Caronia alone had treated in his clinic in Rome about two hundred and fifty cases, of which he obtained up to seventy-five per cent complete recovery. Only very few showed vestibular disturbance or any injurious effect on hearing. He continues treatment up to eighteen months, long after clinical improvement, until the fluids of the body show normal behavior. Some cases received as much as six hundred grams of streptomycin. He considers dihydrostreptomycin not superior to and less active than streptomycin. He injects streptomycin into the meninges and dihydrostreptomycin intramuscularly. Two former patients married and now lead a normal life. Twenty-two children were completely cured and are at a children's home in Ostia.

From Rome we proceeded by car to Florence, via Assisi. It was a beautiful day, and the lands of Umbria and Tuscany were at their best.

Our most memorable excursion out of Florence was by car to Verona, where a medical week was being planned for the last part of July. The Organizing Committee was very anxious for me to come and present an address. In a certain respect I was to be a drawing card to attract large numbers of doctors and tourists. I had the fortitude, however, to decline this invitation, since it would have involved returning to Italy after the Botanical Congress. I also declined the invitation of the American Chest Surgeons to come to Rome in September to receive a special award, as well as of the Congress of Pediatrics in Zurich and of Internal Medicine in Paris, to present addresses.

I could not possibly refuse the urgent request of the group of doctors from Verona to visit that city during our all-too-brief stay in Florence. The Apennines, which we had to cross in the car on our way to Verona, spread before us a magnificent panorama of green-covered hills and valleys. At Bologna we made a noonday stop for lunch in the city famous for its food and had a brief view of the university, especially its magnificent library. The restaurant where we had our meal fully justified the fame of the city. The wine and *tagliatelli alla Bo-*

lognese were especially good. Only in Florence did we taste anything like it. This form of Italian *pasta*, as well as the *tortellini*, left an indelible impression upon our palates. As we were about to ask for our bill, the owner of the place came out carrying a huge volume in his hands. He had just heard from one of the medical people accompanying us who we were. He placed the volume on our table, offered me his hand, and said: "Professor, it is a great honor to have you and your party take a meal at my humble place! I refuse compensation from you, but would be grateful for your autograph in this volume, which is reserved for special guests." I could not, of course, refuse this request.

The visit to Verona was brief, but packed with impressions. First, we saw the city with its Roman forum, Juliet's home and castle with its art gallery. Then we spent the evening in company of a group of local doctors, and the night in the magnificent suburban villa of Dr. Carlo Sigurta, a manufacturer of medicinal preparations. Finally, came a visit from a group of children and grownups whom streptomycin had helped restore to life after an attack of tubercular meningitis. One of the eight-year-old youngsters recited a touching address, composed by the children; it brought tears to the eyes of some of the grownups, including the doctors present. The free translation of this address was as follows: "Illustrious Professor: In the name of the boys and girls of our hospital, I am happy and proud to address myself to you, oh great benefactor of humanity, who have come to honor us today in a very welcome visit. All the children of the world are grateful to you for the grand gift of your medicine, which has given and will give health to many of our brothers. Welcome! Please accept our deepest appreciation and admiration. May God, through the prayer of many innocent ones, help you and guide you always toward great conquests in the field of science, to the advantage of suffering humanity." A bronze replica of the Congrande statue of Verona was presented to me by the group. It carried a very touching inscription: "To the savior of human lives."

I was invited by the medical groups in Florence to deliver an address at the university and to receive a silver medal. Dr. C. Cocchi, the pediatrician who did some of the pioneering work on the use of small doses of streptomycin in the treatment of tubercular meningitis, assured me that it was time for me to stop doing further research work and to discontinue looking further for agents better than streptomycin

for the treatment of tuberculosis. As he put it, "There can be none better; we are now saving sixty to seventy-five per cent of all those that are afflicted with tubercular meningitis, whereas previously we never saved even one sufferer." I visited his clinic and found it packed with people who were coming from all parts of Italy for treatment. The Italian government was planning to build for him a special "tuberculous meningitis-streptomycin clinic."

Thus, first in Spain, then in Rome, and now in Florence, I learned that whereas in the United States and Great Britain, the recovery from tubercular meningitis was only twenty-five to thirty-three per cent, here it was claimed to be sixty to eighty-five per cent. When I told that later to one of my Swedish colleagues and asked for an explanation of this remarkable difference, his only comment was: "There is no doubt the stuff is good; when the British say there is something to it, it means more to me than all the excitement of the Spaniards and Italians." Apparently, early diagnosis and local application have something to do with the higher rate of recovery, although some of my American friends told me that there was not a long enough period as yet following the recovery to make a complete evaluation of the respective results. However, they assured me that the Europeans were probably more correct than the Americans since they recognized the infection sooner and began the treatment at once. After all, the sufferer had only a twenty-one-day chance; the longer the delay in treatment, the less chance of recovery.

From Florence, we proceeded to Toulouse, France, where another round of visits to clinics, addresses, and festivities awaited us. At this university there were three young investigators, who were good friends of our son Byron, whom they had met in various medical centers. They tried hard to make our stay in that famous city of the Midi pleasant. We were taken to Prades in the Pyrenees to hear the concert of Pablo Casals, in honor of Bach's bicentennial. Since the end of the Spanish Civil War, the artist, in protest against the lack of help by the Allies to the Spanish people in their battle against Franco, had refused to give more concerts. He now came out of voluntary retirement to honor the great master of music, at the insistence of many of his friends. On the way back from Prades we spent the night in Narbonne, in the home of one of Byron's friends, where we had opportunity to enjoy real

southern French hospitality. We were impressed by the vineyards and wine cellars of the region.

We spent nearly three weeks in Paris. Here I attended several conferences at the Institut Pasteur and with the Rhone-Poulenc group which had begun to manufacture streptomycin on a large scale; in fact, they hoped soon to reach a production of one million grams a month. They had agreed to pay a royalty on streptomycin comparable to that paid by American manufacturers. I suggested that a foundation be set up to receive these funds, which should be used for encouraging research in microbiology in France. Dr. Jacques Trefouel, director of Institut Pasteur, was nominated as chairman. With him would serve three more scientists and the director of the company. Another organization that was preparing to manufacture streptomycin was invited to join this group. It declined, however, under the flimsy pretext that such action would expose its information to the other company.

Although the organization of this foundation had received universal acclaim, there were certain objections that the funds would probably remain at the Institut Pasteur, that the provinces and the younger investigators would benefit less from them, and that insufficient publicity had been given to the foundation. When I brought these matters to the attention of the directors of the foundation, they asked me to propose, for additional members of the board, anyone I had in mind. Dr. M. Janot, a brilliant young chemist, whom I had come to know well in Geneva, came to my mind. He was immediately accepted as one member, and Prof. Bougnard, formerly of Toulouse University and now director of the Sanitation Institute, who was taking an active part in the organization of research in France and in sending promising young scientists abroad, as another. As to publicizing widely the activities of the foundation, the reply was: "France is so poor now and the funds going into scientific work are so limited that numerous applications will be received, most of which will have to be denied, thus creating a lot of dissatisfaction." It was suggested that the best way to handle these funds at present was to encourage worth-while work in antibiotics and in microbiology in general.

During our stay in Paris, I received several invitations to visit clinics and deliver addresses. The most interesting was the address before the French Tuberculosis Association, which Professor Étienne Bernard

was instrumental in arranging. I also addressed the staff of Leannee Hospital, where I had an opportunity to visit the clinic and speak to many of the patients who were being treated with streptomycin. Among other visits that were particularly memorable, was one at the home of Professor Robert Debré, where were assembled a dozen young collaborators and assistants, who impressed me as highly promising investigators.

We were invited to pay our respects to the Minister of Public Health, M. Pierre Schneiter. We spoke of research in France, especially in the field of medicine and public health; he fully appreciated the fact that France was doing too little, but funds were limited, and he did the best he could to stimulate further research. He was very appreciative of the new foundation and spoke of it as opening a new avenue for collaboration between industry and scientific institutions.

The days in Paris passed rapidly. We did not fail, however, to go to the Comédie Française, the opera, and one or two other theaters. In company of my old friend Dr. Duché and his young son we went to the Loire Valley and Brittany, where we spent a few restful days. In the château country, we took advantage of Prof. Debré's invitation to stay at his country home, a few miles from Tours. Here we learned to appreciate the good wine Vouvray. However, we were greatly disturbed by the outbreak of hostilities in Korea. In this corner of Brittany we could get only local papers, which covered the war news and the American reaction scantily. As we sat on the rocks overlooking the sea, we thought of a faraway world, where people had begun all over again to kill one another, either for an idea or for the mere desire of one group to rule another. "Man's inhumanity to man."

Our short vacation over, we returned to Paris and proceeded from there to Stockholm, to the International Botanical Congress. I took advantage of this visit to meet some of the medical authorities in Stockholm and discuss the various problems that had arisen in connection with the use of streptomycin, the relative value of some synthetic drugs in the treatment of tuberculosis, and their possible utilization in combination with streptomycin. As during previous visits to this hospitable country, we were well received by our old friends and by many new ones. This time, we stayed longer than on any previ-

ous occasion. We gave up a contemplated excursion to the island of Gotland before the congress and one to the "land of the midnight sun" following the congress in order to see more of Stockholm and its environs. Frankly, we were already tired of our journey and were anxious to return home as soon as possible. During the congress itself, I attended only a few sessions, preferring to meet some of the people rather than hear the scientific papers. I gave an address before one of the sessions on the subject of antibiotics. People were still not tired of hearing this subject discussed.

Among the interesting events that occurred during the congress was the participation of a Russian delegation. Until the day before the opening of the congress, nobody knew whether any of these men were coming. Their names, therefore, could not be placed on the formal program. The Swedish organizers were courteous enough, however, to set aside the first evening for the presentation of some of the papers by the Russians. The large auditorium was so packed we could hardly get in. Everybody was awaiting intently for what the delegates would present. Of all the ridiculous things, Russia, a country that has produced so many great botanists, sent to this congress a delegation of Lyssenkoists, who came to defend his theory! Whatever the merits of this theory, it is certainly not new and is based largely on practical observations rather than upon scientific experimentation; worst of all, it is opposed by all prominent geneticists throughout the world. After the papers were presented, many in the audience raised various questions. The Russians did not know any English, those who asked questions knew no Russian, the translator knew little about the subject; as a result, there was considerable confusion. The Russians did not come out the winners. Those who questioned them, however, used little tact and plainly ridiculed them.

The congress was accompanied by several receptions that did not quite compare with those we had attended on previous occasions, especially in 1929. Gone were the luxurious collections of *smörgåsar* dishes that preceded each dinner. But, then, the world had changed. The war had had its effect even upon prosperous Sweden. But, perhaps, it is really our own fault. We have grown up, have been around a lot, and are quite different from the young innocents who traveled all

over Europe in 1924 and in 1929, with a rather meager purse. The world does change, but sometimes the change is in us, and not outside.

The day following the end of the congress saw us in a plane on the way home. Thus came to an end another European journey, so different from the others, but also so much a consequence of the others.

Entering the Zenith

IN JUNE, 1951, the university trustees held a meeting, under the chairmanship of the governor of the state, to decide whether to start building the institute. One or two of the trustees felt that it should be delayed a year or two until prices of construction might go down. This was ridiculous. It was essential to build as soon as possible, no matter at what cost, so that I could still have a few years' time to organize and establish the institute on a firm foundation. More pointed were two questions raised by the governor. One was addressed to me: "What would you do, after the building had been finished, if you had no funds to run it? Would you come to the state for support?" My unequivocal answer was: "I hope that we will not have to do so. What we are planning is of such great magnitude that even if our own resources (from antibiotic royalties) should drop, I am certain that government agencies and philanthropic organizations would come to our support." The other question was addressed to President Clothier: "If there are no funds available for the support of the institute, would the university be able to use the building for other purposes?" His answer was: "Of course we will!" I hung my head. It looked as if we would have to carry the burden alone. My good friend Russell Watson was the only one to voice encouragement: "We will do it!"

The bids were given out and construction was started immediately. In the meantime, our laboratories were moved to new headquarters at the Experiment Station, to a new building appropriately named after my old professor and mentor Dr. Jacob Lipman. I left the laboratory where I had spent thirty-five or so years of my life, where my scientific

work began and attained a point to which I could look with some satisfaction. Had not Pasteur said: "When one's scientific work finds practical application, the cup is overflowing."

Meanwhile, our home life also took a turn. We decided to build a new house, close enough to the institute grounds so that I would not have to do much traveling. I have always felt that everyone should express himself in building a house. One dreams one's whole life about the type of house that one would like to live in. Finally, the time comes when one says: "It has to be done now or never." We decided to do it. It took a great deal of energy and far more money than we had expected to spend. Finally, we gave up the home where we had lived for more than a quarter of a century and moved into a new modern home. It took a bit of readjustment, like fitting a new pair of shoes.

With the building of the institute and of our new home, my outside activities did not come to a halt. Invitations to deliver lectures and receive awards continued unabated. I did all I could, perhaps more than I should.

We had to go to Europe twice in 1951, then again in 1952. These trips involved my attending, as a representative of the U.S. Public Health Service, a conference in Rome, arranged by the World Health Organization. Then, there were questions concerning the manufacture of streptomycin in the different countries. I was hoping to establish foundations comparable to the one in France. I also was invited to receive an honorary degree at the University of Athens, Greece, and the Order of Commander of the Legion of Honor in France.

Finally, I was requested by the Government of Israel to come and advise them on the construction of an antibiotic center. This was a most welcome opportunity, since we were anxious to see the new country. After we had been there fourteen years before, there was left a longing in my heart, a longing comparable only to my father's prayers: "If I forget you, O Jerusalem, let my right hand wither away." I was hoping to see again those ancient shrines where European culture found one of its sources.

In 1952 I delivered a number of addresses in Jerusalem, at the

Weizmann Institute in Rehovoth, at Tel Aviv, and at Haifa. Everywhere we found people hard at work, building a new home. Here was ancient Caesarea, built by the Roman emperors to conquer and control Judea, now, nineteen centuries later, again being converted into a Judean fishing port. And where were those Roman legions of Pontius Pilatus and the other proconsuls?

There were institutions going up everywhere for training and educating people, for establishing great centers of learning. We had a look into the Negev, the southern section, the potential granary and source of minerals. The people must not fail! I felt like telling them, as a member of the tribe of Levi should tell the priest who is to bless the people: *Iasher kochachu!* Let your strength be blessed!

We revisited the Huleh, that peat swamp which I first saw in 1938. Now big machines were at work, trying to divert the Jordan into a new channel, to drain the bog. And all this was being done under threat of the guns of Syrian soldiers sitting across the narrow strip of land. We visited some of the colonies and found people hopeful, in spite of the economic depression; in spite of the tremendously increased immigration, largely of unskilled people from Oriental countries; in spite of constant danger of Arab attacks.

I did not like to leave that country. There is so much that I could have done to help it. A few lectures, some conferences, could hardly suffice. But I had to go back home, where there was still so much to be done. There were still books to be written, there were still the microbes waiting for their home to be built and equipped.

Finally the cornerstone of the institute was laid on May 6, 1952. The new president of the university emphasized in his address that the institute would serve as a milestone in the history of the science of microbiology. In my response, I said:

This occasion is for me one of the greatest personal significance and satisfaction. You may see before your eyes a mass of brick, mortar, and steel, which is beginning to take the shape of a building. I see in it the realization of a lifelong dream, the fulfillment of my highest aspirations. It is with a sense of the deepest humility, therefore, and I must confess not unmixed with gratitude, that I participate in this ceremony. I have devoted nearly four decades to the study of the microbes. During

most of these years I have fought, often a losing battle, for the recognition of microbiology as an independent science, not simply as a handmaiden of medicine or of agriculture. The erection of this building is to me a symbol of the attainment of a goal for which I have fought so long and so hard. It is indeed a great privilege to be able to play some role in bringing about this recognition and this dedication of a great center to be devoted exclusively to the study of the microbes. The microbes inhabited our earth long before man made his appearance; they will remain as long as man survives, and probably much longer. Microbes affect our daily lives, for good or for evil. We came to appreciate the full significance of this fact only in recent years; we are still far from understanding the tremendous potentialities of these smallest forms of life. The laboratories and other facilities of this institute are to be devoted to the study of the microbes, their activities, and their significance in human affairs. Impressive as these material facilities are, however, we must always remember that the real value of this institute will lie, not in its physical plant, but in the minds of the investigators who we hope will come here from all quarters of the globe to study the microbes. To that end let all those who enter here set aside their personal prejudices and personal interests. Let them devote themselves to the advancement of science and human knowledge as a whole, and especially to those phases which pertain to the smallest of living beings, the microbes.

Dr. H. Corwin Hinshaw, now clinical professor of medicine at Stanford University, discussed the present status of tuberculosis therapy at the scientific conference. He said:

Long before man's vision perceived the world of the microscope, his reasoning convinced him that such a world existed. His terminology was different, but when he spoke of the "evil spirits" which caused disease, he seems to have conceived of living entities which could be "cast out," which could pass from man to man. When we learned to see the "spirits" of disease, they did not have claws or fangs or hooves. Yet they competed among themselves, not with mechanical weapons, but with chemical weapons . . . a "chemical warfare," if you please. This concept, enunciated by Pasteur, but brought to fruition by Waksman and his protégés more than by any other group, has not merely revolutionized medical practice, it has profoundly influenced our chance of survival.

It looked as if I would now have to sit and wait until the building was finished, meanwhile rounding up my lifework at the Experiment Station.

But again, there was another surprise in store for me. On October 23, 1952, the Caroline Institute in Stockholm announced that it had awarded me the Nobel Prize in Physiology and Medicine. Thus, the most coveted scientific award was given to a humble worker in the field of microbiological science, primarily in its application to the soil.

For several days, my office and laboratory were bedlam. They were swamped with reporters, photographers, radio and television groups. My time was no longer my own. I had to answer all sorts of questions. In concluding my comments, I said: "My work thus justifies the ancient saying, *Out of the earth shall come thy salvation.*" The newspapers spread the story all over the world. I was immediately requested to supply the source of the quotation. I had difficulty in doing this until several rabbis came to my aid and sent me a quotation from the Apocrypha (Ecclesiasticus), which expressed even better what I had in mind: "*The Lord created medicines out of the earth, and he that is wise shall not abhor them.*"

Congratulations began to pour in from all over the world. Telegrams, cables, letters, telephone calls, in many languages from many countries. They came from my former students, from colleagues, from well-wishers, from tuberculosis sufferers, from foreign groups proud of the accomplishments of an "immigrant boy." Altogether too many. Finally, we made preparations for our trip to Sweden to receive the award.

Some months before, I had promised to go to Japan in December to deliver an address in commemoration of the centenary of the birth of the great Japanese bacteriologist Shibasaburo Kitasato. I could hardly cancel that trip now. But I could make it if, instead of returning from Stockholm after the Nobel ceremonies, we proceeded from there eastward to Tokyo, thus making a trip around the world. It looked difficult, but we decided to do it.

Of the many journeys that we have made abroad, beginning with the Grand Scientific Tour in 1924, either searching for microbes or for information concerning microbes, or attending international con-

ferences, the one in December, 1952, to January, 1953, was by far the most significant from many standpoints. It might be considered the climax of my lifetime association with microbes and the study of their role in the cycle of life in nature and in human welfare.

We decided to go by plane to save time, especially since we would have to leave Stockholm immediately after the ceremonies in order to fill the engagement in Tokyo on December 20, the birthday of Kitasato.

On the evening of December 4, we left Idlewild Airport accompanied by President Kerrigan and one of the vice-presidents, Anderson, of Merck & Co. Byron was to follow two days later from Boston.

Although it was late at night when we arrived in Stockholm, we were met at the airport by a delegation representing the Nobel Foundation and the American Embassy and by our old friend Prof. Nanna Svartz of the Caroline Institute. There were more newsmen to take photographs and ask questions. At the suggestion of the American Press attaché, most of them gathered in the Grand Hotel after we had registered and gone to the suite of rooms assigned to us. It was nearly midnight when we met in the parlor some fifteen or more reporters representing various newspapers and news agencies. They were interested in my past, my present, and especially in my work on streptomycin and other antibiotics. After midnight, we had a long session with Mr. Sjöberg, representing the Swedish Foreign Office, who was attached to us during our total stay in Stockholm, and with Prof. Svartz, who remained to discuss with us in further detail the plans for the forthcoming events during the week ahead of us.

The next morning, Mr. Sjöberg arrived early to have breakfast with us in our rooms and to discuss further the various plans for the social and scientific activities. Immediately after breakfast we paid a visit to the office of His Excellency Marshal B. Ekeberg, director of the Nobel Foundation, to leave a written acceptance of an invitation to a reception that he had extended the Nobel Prize winners.

At three o'clock that afternoon, accompanied by Mr. Sjöberg and Prof. Svartz, we proceeded to Uppsala, where a dinner had been arranged by Prof. Nilsson, dean of the College of Agriculture. We made a tour through the laboratories of the Institute for Microbiology, which was carefully planned and built by Prof. Ch. Barthel, formerly bac-

teriologist at the Swedish Experiment Station. Many of our old friends were present. First there was Prof. Barthel himself, now eighty years old and who lives in complete retirement. It was a particular pleasure to see him again because on our previous visit to Stockholm, at the Botanical Congress in 1950, he had been too sick to see us. We first met him in 1922, when he visited me in New Brunswick, where he saw some of our early work on antagonistic relations among microbes. Since then, we have remained good friends and have exchanged constant information in our search for microbes. There was Dr. G. Giobel, my oldest student in Sweden, who took his Ph.D degree in our laboratory in 1925 and who has since been connected with the Swedish Agricultural College. Also at the dinner were representatives of the Ministry of Agriculture, of the college and university, as well as the American agricultural attaché and his wife. It was a splendid gathering, where we felt quite at home among good friends.

The following evening a dinner was given by the National Anti-tuberculosis League of Sweden in the famous old restaurant Den Gyldene Freden (Golden Peace), which is now the property of the Swedish Academy. The Queen of Sweden, who is president of the society, was represented by Countess Bernadotte, a very charming American-born lady, widow of the King's nephew, who was peace negotiator in Palestine and who paid with his life for his efforts to establish peace between the Jews and the Arabs in the Holy Land. The most outstanding tuberculosis experts of Sweden were at the dinner, notably Professors Lundquist, Wallgren, Kristensen, and Heldstrom, and their wives, as well as my old friend Dr. Davide, the bacteriologist, and his wife; Dr. Malmberg and his wife, Prof. Svartz; and supreme court judge Nils Beckman, vice-president of the society. There were toasts by Dr. Lundquist and by Mr. Beckman. I responded by telling of three historical events in my career: my first meeting with Dr. Svartz, in Stockholm in 1946; my visit to Dr. Löffler, of Zurich, Switzerland, in 1947; and the visit that very morning of a father and his young daughter, Eva Hallström, who five years before was saved by streptomycin from certain death from tubercular meningitis. Hers was said to have been the first such case in Sweden. Little did I dream that this story was later to arouse great excitement in the world press. We returned to our hotel late at night to await the arrival of Byron.

On December 8, early in the morning, we left by special train for Strängnäs, some fifty miles outside Stockholm, to participate in the official opening of Kabi's streptomycin-producing plant. Some seventy representatives of government, industry, science, and medicine were there. Crown Prince Bertyl represented the King of Sweden. A detailed tour of inspection through the plant, which was magnificently designed and built, was followed by a luncheon. Addresses were delivered by the president of the company, Mr. Isaksson; the president of Merck & Co.; the Crown Prince, and myself. The Prince proved to be a charming person, who has captivated the hearts of his countrymen. He is outspoken and simple in manner. He was amazed when I asked him, during the inspection tour, to smell a culture of the streptomycin-producing organism and note the odor of freshly plowed soil. In my address, I emphasized the importance of collaboration between science and industry, on the one hand, and between different countries, on the other.

During the previous two weeks, Byron had succeeded in learning sufficient Swedish to carry on a fair conversation in that language. This caused a great deal of comment, and I imagine that he will become quite a legend there.

We returned to Stockholm in time for another dinner given by Dr. A. Wallgren of the Nobel Committee on Physiology and Medicine, who was to present me formally for the award. This dinner was attended by a number of prominent members of the Caroline Institute, including the Rector. The host spoke of the numerous social obligations of the Nobel laureates, warned of the ease of breaking down under them, and advised prudence in meeting them. In responding, I praised the hospitality of the Swedish people and pointed out that we had been in Stockholm on several previous occasions and that we had many friends there, as well as a few former students and collaborators. After dinner, Dr. Wallgren took me into his private study to discuss various matters pertaining to the official ceremony and his address. I, in turn, outlined what I was going to present in my own response to the award.

On December 9, various reporters came to verify a rumor that we were planning to go from Stockholm to Tokyo by way of Russia. I convinced them that neither my own country, the United States, nor

the Soviet Union would be very happy about such a trip and that we were actually taking the long airplane route via southern Europe and southern Asia. The reporters also wanted an advance story on my forthcoming address and information about my general impressions of Sweden. At the invitation of the Scandinavian American Foundation I went to a radio station to make a ten-minute broadcast, to be used in the United States, on the subject of streptomycin.

In the afternoon, we attended Marshal Ekeberg's reception at the Nobel Foundation House, where we met most of this year's Nobel Prize winners and some of former years. Among the latter, we had a very pleasant conversation with the writer Lagerquist, former prize winner in literature, my old friends Heymans of Ghent and Von Euler.

We were particularly pleased to meet Alfred Nobel's nephew, Gustav Nobel, and his two sons and their wives. Gustav was born in Russia, where he had spent most of his life. He left at the time of the Bolshevik Revolution. He spoke excellent Russian, and we exchanged many reminiscences. When I mentioned that the wife of one of the professors, who also spoke Russian, had just warned me to be very careful in using the language, so as not to expose myself to any undue accusations, he said: "You and I have nothing to fear."

December 10 was the busiest of all days. It began with visits from a number of reporters, including the *Politiken* in Copenhagen and another from *Paris-Match* of France. The news had spread of the visit of Eva Hallström and her father, and the newsmen all wanted the details. Later, I had to go to the Concert Hall for a rehearsal of the main celebration. Here I met all the new Nobel Prize winners and learned something of their travel adventures.

The Solemn Festival of the Nobel Foundation took place in the Concert Hall at four-thirty that afternoon. At the sound of trumpets, the prize winners marched into the hall and up on the platform, accompanied by music especially composed for the occasion. The hall was packed. The King, the Queen, and three other members of the royal family sat in front facing us. Behind us on the platform were numerous professors from different universities and institutes, former Nobel Prize winners, and other dignitaries in full regalia. As each candidate was called and his citation read, he marched down toward

the King, who came forward to meet him. The King shook hands, presented him with the medal and diploma, and said a few courteous words. The prize winner then bowed, stepped back toward the platform, walked up, bowed again to the King, and went to his seat amidst thunderous applause. After the ceremonies, many came up to congratulate us. The reporters found little Eva Hallström and brought her to be photographed presenting me with a bouquet of flowers. This was the "human-interest story" that later was responsible for numerous letters from tuberculosis patients.

In the evening, we attended a formal banquet at the Town Hall. I had some difficulty with my attire, but, with Byron's help, I managed. Prince Wilhelm escorted Bobili, who sat between him and the King. I escorted Princess Sibylla, the mother of the Crown Prince, who sat on my right. On my left, was Mrs. Erlangen, the wife of the socialist prime minister. I tried to avoid politics, which was not especially difficult in a democratic country like Sweden. The smiling face of Prince Bertyl, who was sitting across from me, gave me added courage. Preceded by a fanfare of trumpets, the Rector of the university delivered a brilliant address, both in English and in French. He was followed by Mauriac, who spoke beautifully. My turn came next.

In responding, I said, among other things: "From the moment he is born to the moment he dies, man is subject to the activities of microbes. Some are injurious and others are beneficial. The latter have made recently a great contribution to mankind by producing the antibiotics. There has thus been placed in the hands of the medical profession one of the greatest tools for combating infectious diseases of man and of animals." In concluding my remarks, I quoted from the words of Alfred Nobel, the founder of the awards: "The advance in scientific research and its ever-widening sphere stirs the hope in us that the microbes, those of the soul as well as of the body, will gradually disappear, and that the only war humanity will wage in the future will be one against these microbes." After the banquet, we went down into the larger hall, which was full of marching and singing university students.

On December 11, we went to the Nobel Foundation to sign various important papers. We invited some friends, including my former

student Dr. Melin and his wife, to lunch at our hotel. In the evening, a formal dinner given in honor of the new Nobel laureates by the King and Queen of Sweden in the castle was a momentous occasion. Bobili was escorted by Prince Bertyl and sat between him and the King. I escorted Mrs. Erlangen and sat between her and Mrs. Stevens, the wife of the British Ambassador. The guests were limited to the Nobel Prize winners and their wives, some former Nobel Prize winners, the ambassadors of the respective countries and their wives, some professors of the various institutes and universities, and court attendants. Although the Court was very ceremonious, the King and royal family were very simple. There were no toasts. The wines, four kinds, were all poured out at once to be taken as one pleased; there was even a glass for water at the table, since the King drinks no wine. After dinner, we marched out into the large, beautifully lighted hall, with fires burning in the fireplaces, and mingled very informally with a very cheerful company. Now I had a chance to talk to some of the other Nobel Prize winners, to the Prime Minister, and especially to the King. He was very much interested in the utilization of streptomycin and other antituberculosis agents; we talked about antibiotics in general and especially their use in the treatment of tuberculosis. For forty years the King had been chairman of the antituberculosis work in Sweden. He is very well informed, extremely intelligent, and highly democratic. The people have a very high regard for him and justifiably so.

On December 12, at noon, I delivered my Nobel address, "Streptomycin—Background, Isolation, Properties, and Utilization." It lasted exactly forty-five minutes. The auditorium in the Caroline Institute was full. In the front sat Princess Sibylla and her attendants.

The lecture was followed by a luncheon given by some of the members of the Caroline Institute.

In the afternoon, in honor of the American winners of the Nobel Prize, the American Ambassador, Mr. Butterworth, gave a reception at our embassy, a beautiful mansion adjoining a park. At his request we arrived early to join him and his wife in the receiving line. The embassy staff, comprising many attachés, also arrived early with their wives. They were soon followed by foreign ambassadors and their wives, members of the Nobel family, a number of professors and

their wives, and resident and visiting Americans. We must have shaken hands with three hundred persons. It was quite a task. We spoke in a variety of languages, mostly English and French. Several persons who had benefited from the use of streptomycin were profuse in their gratitude. We had to leave promptly at six to dress for another formal dinner, given by Prof. Svartz and her husband, Prof. Malmberg.

The dinner was a magnificent affair, arranged for some twenty couples, mostly professors of the Caroline Institute. The hosts delivered excellent addresses in greeting us. There were toasts and cheers in the old Swedish manner. On behalf of the other guests, Prof. Bergstrand, Rector of the institute, and I responded. Following the dinner, we spent an hour or more in conversation with different members of the group. At eleven-thirty we had our first Lucia celebration at Dr. Svartz's home. Hot wine (*Glückvine*) and coffee were served, and the ladies sang "Santa Lucia." We left at midnight.

Early the next morning, we were awakened by a group of white-clad girls singing "Santa Lucia." The leader, bedecked with a crown of lighted candles, placed on my lap a tray of coffee and cakes. This Lucia celebration, held annually just twelve days before Christmas, appears to have been imported from Italy and superimposed upon an ancient celebration of the northern peoples. We kept hearing the song all day.

Later, we met with representatives of the management and scientific staff of the Kabi Company to discuss the future of antibiotics. Then we were visited by representatives of a society comprising twenty-three thousand former tuberculosis sufferers, who presented me with a beautifully engraved, very heavy glass vase, suitably inscribed. I was deeply touched.

That evening we attended the annual Lucia celebration of the science students of the University of Stockholm. There were speeches and songs. All the Nobel Prize winners in science were initiated into the student fraternity "Order of the Frog."

On December 14, early in the morning, we bade good-by to friendly Stockholm and boarded a direct plane for Tokyo. Several of our Swedish friends and Byron came to see us off. Byron left that afternoon by plane for Boston.

Circling the Globe—Visiting Japan

WHEN we left Stockholm, it was snowing, and the plane was an hour late in taking off. About two hours later when we landed in Copenhagen, the snow was changing to rain. It was very pleasant to be met there by our old friends the Bloms. Dr. Blom, who had worked in my laboratory some twenty-five years before, was now chief chemist at the Tuborg brewery. He brought with him a case of Tuborg's fine dark beer, which we took with us on the plane and eventually shared with some of our Japanese friends.

The plane left Copenhagen in the rain. After stops in Frankfurt and Geneva—where a number of Far Eastern delegates to some of the United Nations agencies entered the plane on their way to the Near and Far East—it was still raining when we landed late that evening in Rome. Here a group of old friends braved the weather to greet us, congratulate us, and wish us a *bon voyage* eastward.

In the middle of the night the plane made another stop in Cairo. But we had gone to bed, and all we heard of the Egyptian capital was a lot of noise and excited conversation mostly in Arabic. Soon, however, we were off straight eastward. We arrived at Karachi about noon the next day. Here a warm sun welcomed us. The bright sun and dry air were a pleasant interruption of the monotonous journey. We walked for nearly an hour around the clean and well-kept station, basking in the sun. In fact, this was the only bit of sunshine we saw on our whole transworld journey.

Our stop in Karachi was not without its humorous side. As we stepped off the plane, we had to produce our health certificates before

we could enter the station. The Pakistan public health official had never heard of our home state, New Jersey. When I told him that it was in America, he said with a gleam in his eye: "Oh, South America!" I assured him that it was one of the United States, even if not so large or famous as New York, Texas, or California.

Our next stops were Rangoon, in the middle of the night, and Bangkok the following morning. We now flew over Indo-China, Thailand, Burma. We could see numerous rice fields, quite a new experience to us, occasional habitations, and a variety of topographic changes.

We arrived in Okinawa late in the afternoon of the third day. At the airport, we saw some American soldiers who were attending to their own business and paying little attention to us. Only a few natives were around. As the plane left the island, we watched the numerous lights of the habitations.

We were thoroughly exhausted upon our arrival in Tokyo late at night, December 16. We were met at the airport by an immense crowd, including several scientific delegations from different universities and institutes. A number of children, dressed in beautiful kimonos, presented us with huge bouquets of flowers. There was a group of resident Americans, including the cultural attaché of the American Embassy, Mrs. Williams, with an official embassy car. There were many professors of Keio University, our host institution, including our own official translator and guide, who were to be with us all through our stay in Japan. Bulbs were flashing, cameras were clicking. Loud conversation was carried on in both English and Japanese. It looked as if we would never be able to break away.

Finally, largely through the efforts of the energetic Mrs. Williams, who almost literally cut a swath for us to pass through, we reached our Tokyo headquarters, the Imperial Hotel. Here a large number of reporters had congregated. But we were too tired to see them and asked them to return the next afternoon for a general press conference. Mrs. Williams and our two Japanese guides remained with us for about an hour to acquaint us with certain arrangements that had been made for our next few days in Japan.

Early the following morning, while Bobili remained in the room to rest up and arrange things for our stay in Japan, I was taken to

the Kitasato Institute to pay my respects to its president, the son of the founder, and to the aging vice-president, Dr. T. Kitashima, who was honorary chairman of the Kitasato celebration. I also visited the Robert Koch shrine, where the late Dr. Kitasato used to render his daily prayers. I had an opportunity to see some of the laboratories, especially that of Dr. Hata, the son of the famous collaborator of Ehrlich on salvarsan, now working on antibiotics. I was then taken to Keio University, the oldest university in Japan, which was largely responsible for inviting me to make the present trip.

We had an interesting lunch in the dining rooms of the *Yomiuri Press*, publishers of the largest daily newspaper in Tokyo. Sitting between the president and the editor in chief, I had an opportunity to learn something of conditions in Japan, its relations to its neighbors, and the general economic and political picture of the country.

Soon after lunch I met the press, representing many Japanese newspapers and radio stations. With the aid of my Japanese translators I tried to answer their numerous questions. The evening ended with a sukiyaki dinner in a Japanese restaurant. Here our stockinged feet became thoroughly bent and we had our first lesson in the use of chopsticks.

The American Embassy telephoned that an audience had been arranged for me with the Emperor of Japan, and that I was to be presented personally by the American Ambassador, Mr. Murphy. Then came the question of suitable clothes! I had no striped trousers. After the Stockholm ceremonies, my cutaway coat had been dispatched back to the U.S.A. with Byron. The American Embassy and our good Dr. Katow immediately got busy trying to buy or borrow the necessary outfit. Before the evening was over, a coat and trousers belonging to someone rich enough to possess them were delivered to me. They were about my size. The coat felt a bit too large, but I was assured that everything "looked perfect."

Early next morning, the two telephones in our rooms began to ring steadily. Poor Dr. Katow, who occupied an adjoining room, was constantly running in and out with all sorts of messages. Finally, the American Embassy car appeared to take me to the palace. Since the main palace burned during the war, the Emperor has been occupying a smaller place. After passing through several roomfuls of bowing offi-

cials, we were led to an inner room, where I was presented to the Chief Imperial Chamberlain and the official court translator.

The Emperor soon appeared. He was short, about fifty years old, wore glasses, and had a mustache. He very cordially welcomed me to Japan. He wanted to know all about the present status of tuberculosis therapy and the future potentialities. He expressed sincere hope that the Japanese medical profession and universities would benefit from my visit and that I would carry away with me from Japan information that would be of help to me in my own future work. He further emphasized the hope that the exchanges of information between us would help to strengthen the ties between our two countries. During the conversation, his face would light up with a smile, and he would bow to express satisfaction. It was my impression that he understood English well, but that the use of an interpreter enabled him to formulate his questions more properly. He is himself interested in science, especially in marine forms of life, and has written a book on a group of hydrozoa. The interview lasted about thirty minutes.

After the interview, Ambassador Murphy sent a car to the hotel to fetch Bobili and invited us to lunch at the American Embassy, where we met the younger brother of John Foster Dulles, the future Secretary of State. He and his wife were visiting their son, who had been badly wounded in Korea by shrapnel in the head and was still unconscious in the Army hospital. They were told by the physicians that he would survive largely because of the antibiotics.

That evening we took an interesting walk to the Ginzo, the leading street in Tokyo, looking in on a number of "hot spots."

The celebration day came on December 20. Prior to my main address, a conference was held in the offices of the Yomiuri Press. There we met Princess Chichibu, the wife of one of the Emperor's brothers, who was very ill with tuberculosis and some secondary complications. We also met another brother of the Emperor who is vitally interested in the eradication of tuberculosis in Japan. We were told that before the war the death rate from tuberculosis in Japan was two hundred thousand annually, or twenty-four to twenty-eight per ten thousand people. It is now only seventy thousand annually, or eight to nine per

ten thousand. They like to believe that this drop in mortality is due to the use of streptomycin.

The official celebration began at one o'clock in the lecture room of the Yomiuri Press. Preliminary addresses were delivered by the president of the Diet, by several ministers, by the president of Keio University, members of the Kitasato Institute, and the German Minister in Japan, who spoke of Robert Koch and Kitasato. Dr. Ushiba, a young Japanese bacteriologist, recently returned from a stay at Harvard Medical School, translated sentence by sentence my address, "The Changing Concept in Microbiology." The audience of more than one thousand included the brigadier general of the Medical Division of the American Military Government and many other members of the American military and civilian staffs as well as a number of foreign delegates, including my old friend Prof. K. F. Mayer of the University of California.

Later in the evening a formal banquet was held for some six hundred and fifty at the Imperial Hotel. Addresses were delivered by close associates of Kitasato, by government and university delegates, by American Ambassador Murphy. I was the last speaker. Both the ambassador and I emphasized the solidarity of science, its international nature, and its important role in unifying the peoples of the world. The ambassador paid me the very high compliment of emphasizing that the celebration was in both Kitasato's and my honor.

The next day, Sunday, we were taken outside the city to visit a Miss Kaji, a tuberculosis victim, whose health was improving as a result of treatment with streptomycin. Her family was most appreciative.

Then there was a luncheon at the American military personnel headquarters, given by my old college friend Janeway, whom I had not seen since his graduation from Rutgers. He had been in Japan for five years, in charge of the Far Eastern division of the American Red Cross. Here I obtained a fairly clear picture of the present Far Eastern situation, especially as seen by American medical men.

In the evening, we were entertained by a Japanese chemical company, at a dinner in an ancient Japanese restaurant. We were served, for the first time, a typical Japanese meal and were entertained by a

group of geisha girls, who danced and sang most pleasantly. We had to leave the dinner early for the Kabuki Theater, where we saw a bloodcurdling play, in the ancient Japanese tradition. Two prominent Japanese scholars translated and interpreted the play to us.

I spent a good part of a day at the patent office, where I talked with a group of examiners interested in handling antibiotic patents according to Japanese law, meeting with manufacturers of streptomycin to discuss various aspects of the problem arising in Japan. Of particular interest was the forthcoming patent on streptomycin, which would be issued to us, the issuance of license agreements, and the possibility of establishing a foundation in Japan for microbiological research. We discussed various legal and technical aspects.

In order to give a lecture at Tohoku University, I had to travel about eight hours to Sendai City, spending a day there and returning the following night. I was accompanied by Dr. Ushiba, my official translator, Dr. Hata, Dr. Nagano, and one of the editors of the Yomiuri Press. I spoke on "Antibiotics and Chemotherapy." There was an opportunity to visit various departments of the university and the local American Army hospital. A great deal of work is being done in the former on actinomycetes and on production of various antibiotics. Several new substances have already been isolated. The Governor of the province gave a reception and dinner in the evening in a classical Japanese restaurant, and I was presented with a very handsome lacquered salad set.

The following day we had dinner at the home of Prince Mikasa, the youngest brother of the Emperor, accompanied by our friend Dr. Katow and his wife. Soon after the formal introduction, I asked the Prince whether he, like the Emperor, was interested in scientific research. His reply was that he was interested in history, especially ancient history. Upon further questioning, I discovered to my amazement that he was studying Hebrew and reading the Old Testament in the original tongue. He has a fine analytical mind. We discussed Buddhism, Shintoism, and Christianity, the present religious tendencies in Japan, and the possibility of the Japanese accepting a new religion. Although I am not a missionary by nature, I could not help but think of the various conversions to Judaism or Christianity of former royal houses, notably in Eastern Europe and Central Asia.

We paid little attention to the very low temperature in the room or to the modest quarters in which the Prince, his charming wife, and their husky children lived. The dinner was in Japanese fashion, the Prince and Princess themselves preparing and serving the sukiyaki. We drank plenty of sake and were engaged in continuous conversation. Dr. Katow later commented that the reception, lasting nearly three hours, was the longest the Prince had ever given to foreigners.

Christmas day was spent in Tokyo, mostly in restaurants, eating and conversing. At noon, we were invited to an excellent luncheon given by the Minister of Public Health and Welfare, at the official home of the Premier. The minister greeted us cordially and announced that the government had recommended that the Emperor confer upon me the Order of Merit of the Rising Sun, one of the highest decorations, rarely conferred upon a foreigner. We sat in chairs and had a good Western meal. This was later followed by a garden party given by the Japanese Medical Association in the Happon Gardens. We witnessed the tea-drinking ceremony, walked through the magnificent park, and settled down to a fine repast of half-Western, half-Japanese food. We were presented with a large collection of Japanese prints by the president of the association.

At five o'clock we had to hurry to another dinner, this time in typical Japanese style, given by the Governor of Tokyo. Afterward we had time to visit our old friend Leonid Kreuzer, the well-known orchestra conductor, whom we had not seen since 1934, when he spent several months in New Brunswick after he left his position in Berlin. He had had a stroke recently and was in bed. He was very much excited to see us. He had married a Japanese girl pianist and built himself a beautiful modern house. We enjoyed a very pleasant visit with him.

The following day I visited the laboratories of the Agricultural College and Experiment Station of Tokyo Imperial University. It was so cold that I had to wear my overcoat and hat most of the time. We talked largely on the subjects of humus and fermentations, although the antibiotics were not forgotten. We ended the visit by going to a magnificent university library, where the president entertained us at a Japanese-style lunch. We talked mostly about science and religion, but not politics. Apparently, the people in Japan are ready

to make some important changes, but they do not quite know themselves what these will be.

Immediately after lunch, I proceeded to the Ministry of Education, where the minister handed me a diploma and the insignia of the Order of Merit, in two forms, one for formal wear and one for informal. Pictures were taken. That evening, we were entertained at a magnificent dinner by the Antibiotics Association of Japan. Again there were geisha girls, performers, acrobats, dancers. We drank innumerable cups of sake, listened to toasts, and found it all quite interesting. We returned to the hotel about nine o'clock to be greeted by a large delegation of Jewish residents of Tokyo, mostly of Russian origin, and the newly appointed Israeli Ambassador to Japan.

The following day, we went to visit the National Museum, where we admired fine collections of Japanese prints. The director opened the museum especially for us, and he and his staff, including some American-educated art experts, guided us through their treasures. We learned a good deal about Japanese history and art. We ended by drinking tea in the office of the museum, hugging a coal stove. From there we went directly to the National Academy of Sciences to a luncheon by the Japanese Science Society. Here we had an opportunity to meet some outstanding scientists in fields other than biology and medicine.

There was barely enough time to return to the hotel, rest a few minutes, and pick up my slides and manuscript for my second address in Tokyo, this time on the subject of antibiotics, delivered at Yomiuri Hall before a packed audience. At the end I answered several questions that had been presented in writing. It was cold, and I stood over an electric hot plate in front of the desk.

In the evening, an excellent Japanese dinner was given by the Yomiuri Press. Geisha girls, with their friendly "*Dhoza*," or please, continuously filled our cups. There were also dancing and singing.

The next morning, with several Japanese friends, we left early by car for Nikko, the famous historical Buddhist shrine center of Japan. We were met at the hotel by the mayor of the city, who introduced a capable guide who had made a detailed study of the history, religion, and folklore of the region. We were soon joined by Mr. Kanya, owner of the hotel, and by a Buddhist priest. We visited sev-

eral temples, were shown into the holy of holies, bought some charms, and discussed Shintoism and Buddhism. The first is simple enough, since it is said not to be a religion at all but merely a mode of life. Buddhism proved to be more difficult to understand, since the Japanese sects have gone a long way from the teachings of Buddha. Finally, as we were leaving the shrine, we were joined by a young priest, who accompanied us to the hotel, had dinner, and spent the evening with us. Unfortunately, he spoke very little English and what he said appeared to lose most of its substance on translation. Our translators, Buddhists themselves, appeared to know little of the manifold aspects of its philosophy. Fortunately, at dinner we ran into Mr. Turner and his family. As counselor of the American Embassy, he had met us at the ambassador's home. Having spent more than twenty years in the Orient, he knows well the various religious philosophies, and he enlightened us on many of the problems involved, which were not all easy to comprehend.

We had a very pleasant room facing a magnificent panorama and were awakened quite early by the sun rising above the famous mountain. We understood that when the Buddhist priest came here about a thousand years ago he spent several years going up the mountain in search of God, who was sitting there waiting for him. We left early for the top of the mountain to visit the lake and pay our respects to another shrine.

On our return that afternoon to Tokyo, we were handed a cable from Byron reading: "Born today eight-pound boy named Peter. Joyce and baby well." Thus a grandson was born to us.

We gladly accepted the invitation of Dr. Abe, dean of the Medical School at Keio University, to have dinner with him and a few other guests at a famous restaurant. Here we were entertained by a group of the most original geishas we had so far seen. In our party were Professor Tamiya, the bacteriologist, and Mr. Imaguwa, a former minister of agriculture, now a senator, who spoke of his friend Frank Booth, a Japanese-born American and a graduate of Rutgers, now seventy-two years old, who was at one time very wealthy and was responsible for the Russian-Japanese fishing agreement.

Early the next day, we left by car for Hakone Hot Springs, where we did a lot of sight-seeing and spent the night. The next day, we

were driven by car to the railroad station, crossing a beautiful chain of mountains and admiring some excellent views of Fujiyama. The train brought us early to Kyoto, where we were met by a delegation from various universities at which I was scheduled to lecture. A car took us directly to Nara, the famous shrine city and oldest capital of Japan.

This was New Year's Eve. To welcome the New Year properly, we went to Osaka, looking for a night club. All of them were closed, or about to close, or were packed with American soldiers and their Japanese girls. We finally gave up and, close to midnight, started back for Nara. We greeted the New Year in the automobile, midway between Osaka and Nara.

New Year's Day was spent in sight-seeing in Nara, visiting ancient Buddhist temples, examining wooden carvings more than thirteen hundred years old, looking at big Buddhas, talking to priests, admiring parading crowds going to the Shinto shrines, especially the groups of women and little girls dressed in pretty kimonos, receiving delegations of patients from local sanatoriums, as well as the mayor of Nara. It was indeed a charming spot. No wonder the King of Sweden urged Bobili not to miss Nara.

From Nara we returned by car to Kyoto, where we spent two days. We toured the Emperor's gardens by special permission and visited more Buddhist temples. We also visited a rock garden, without water and without much of a garden, except for symmetrically arranged stones (positive and negative, male and female, supplementary forms). A Buddhist priest who presided here invited us to his home, where we drank tea and ate sweets and tangerines. Next we went to the silver pavilion, famous for its garden and spring, where a crowd was admiring a synthetic sea made of sand.

I delivered a lecture one afternoon at Kyoto University. That evening, the mayor entertained us at a Japanese restaurant where the geisha girls were dressed in holiday kimonos, typical of New Year's and characteristic of old Kyoto. There were music on the shamisan and dancing both by groups of men and of women (not mixed). The whole city of Kyoto proved to be most charming.

On Sunday, January 4, we left by car for Osaka, where we stopped

at a nice, modern, warm hotel. Immediately after lunch, the local antibiotic society held a round-table discussion, and later I gave the usual lecture before a tightly packed audience of more than twelve hundred.

In the evening, we were entertained by the Governor of the province and the mayor of the city. The American Army was well represented at both the lecture and the dinner. The American Consul General and several American women also attended. The genial Dr. Abe offered to take us afterward to a leading cabaret, to compensate us for the lack of a New Year's Eve celebration. But we preferred a walk through the city with some of our Japanese friends. We ended the extremely busy day by eating part of a huge cake presented us by a baker who had been cured by streptomycin. This reminded me of the Tokyo tailor who wanted to make me a suit of clothes to express his appreciation for streptomycin. I had a hard time convincing him that though I greatly appreciated his offer I needed no extra suit.

On January 5, we left early by train for Nagoya. When we arrived about noon, it was raining. The hotel was cold and our room, directly over a jazz music store, was very noisy. But the hospitality of the people was so great that it made up for everything. That afternoon, prior to my lecture at the university, there was a special showing of a "streptomycin film," made in our laboratory by the USFS some four years before. It was interesting to see some of my students and assistants again in this faraway city on the other side of the world and listen to the Japanese story on streptomycin. Following the lecture before a large and enthusiastic audience, we were taken to a cloisonné factory where we watched the process of shippo-enameling. The handwork was beautiful and the craftsmanship masterly.

In the evening, we attended a grand dinner with the governor of the state and his wife, the mayor of the city, the American Air Corps General, and other members of the security force and diplomatic corps. In response to greetings by the Japanese officials I paraphrased the admonition, "Never say *kekko* (beautiful) before you have seen Nikko," as "Never say you have seen the hospitality of Japan before you have been to Nagoya." Finally, the American Consul General made an address, both in English and in Japanese, which was very well

received. We were presented with several magnificent gifts and given a hearty farewell; then we went to our cold rooms to rest an hour before taking the midnight train for Tokyo.

A large group came to the station to bid us good-by. The American consul insisted that our visit had helped in cementing American-Japanese friendship: "If it were in my power to give out medals, I would certainly present you with one."

The train was comfortable, and we arrived in Tokyo early the next morning. Here we found a letter from Mrs. Williams of the American Embassy concerning an audience that Bobili and I were to have with the Emperor and Empress. Arrangements had been made for our introduction by the ambassador and his wife. Unfortunately, a second letter was also there stating that owing to the death of Prince Chichibu, the Emperor's brother, all official engagements had been canceled.

After a luncheon of the Tokyo Rotary Club, we went to Keio University, where I was given an honorary degree of Doctor of Laws, the first ever granted by that university. There was a great assembly of professors and guests. A famous art collector presented Bobili with a series of eight color prints by Hiroshige, the famous Japanese artist of the early nineteenth century. They are said to be extreme rarities. I was interviewed over the radio Voice of America concerning my impressions of Japan and gave another broadcast over Radio Japan. Various people, bringing gifts and asking all sorts of questions, came to see us at our room in the Imperial Hotel.

In the evening, we were treated to a farewell dinner, Western style, at the hotel, by our friends of the Kitasato Institute and the Keio University. The aged Dr. Kitashima gave a very fine address. There were detailed discussions and comparisons of Robert Koch's visit in 1908 and our present one. The first appealed to a small group of scientists, and the second to the people as a whole; this explains the very great attention paid to us by the press and the Japanese people. We said good-by to all our friends, although many insisted they would come to the airport to bid us a final farewell.

The last day in Japan was a busy one. People began to drop in early in the morning. My old friend Dr. Oshima, of the Takamine days, who is now director of laboratories of the Bureau of Fisheries,

made a special trip from Hokkaido to see us. The next caller was Frank Booth, Rutgers 1903, born in Japan in 1880, whose father Eugene, Rutgers 1876, went to Japan as a missionary. We spoke of Mr. Murray, a former Rutgers professor who came to Japan in 1871-1872 and was responsible for developing the educational system there and for sending various Japanese students to Rutgers. Mr. Booth had his own ideas about Japan and its problems, gleaned from a long and busy life in the country. One of his activities was the organization of a canning industry for fish products, especially salmon and crab.

The departure from Japan presented a rather moving scene. A number of our Japanese friends came to the airport bringing flowers. Fortunately, the terminal was too crowded for farewell speeches. Good Dr. Abe, who was there with his wife and children, had tears in his eyes when he said good-by. My faithful companions throughout our stay in Japan, Katow and Ushiba, were there along with representatives of the Yomiuri Press, of the Institute, and of Keio University, and several families of tuberculosis sufferers.

It was snowing as we entered the plane, but we left on time, after posing for a final series of photographs. The roar of the motors soon drowned out our voices. Thus we said good-by to a beautiful country, to a charming and hospitable people. We had to promise that we would come again!

The trip to Hawaii lasted about seventeen hours, with a single stop during the night at Wake Island. A large delegation from the local medical society, the tuberculosis group, the Army hospitals, and the university met us at the airport in Honolulu. Both of us were decorated with numerous *leis* of fresh multicolored flowers, so that we became for a time walking aromatic flower collections. The custom examiners took care of us immediately and sent us on our way with all our bundles, boxes, and bags full of all sorts of dolls, knickknacks, and other presents from Japan.

I delivered a lecture before a local medical society and visited the university at the invitation of its president, Dr. Sinclair.

The four days in Hawaii passed very quickly. Col. Aaron, who had headed the ROTC at Rutgers for a year and who was now teaching mathematics at the Kameamea School took us to dinner at one of the military clubs. We visited the great Army hospital, Tripper's

General, and did considerable sight-seeing in company of Dr. Sinclair and one of the professors. We also had a chance to visit the local museum, where we greatly admired the Japanese prints, since we could now understand their full significance. We also had a few minutes free to sit by the balmy sea.

Our stay in Japan gave me ample chance to see something of Buddhist religion and philosophy. I had asked many questions of my Japanese friends and of Buddhist priests. The answers were extremely unsatisfactory. The Japanese themselves were uncertain, not to say confused, about some of the principles. My friend Katow kept emphasizing the negative and positive, the male and female, and supplementary factors. Dr. Moore, of the University of Hawaii, who seemed to have made a study of Buddhist philosophy, emphasized the oneness of the universe, the transmutability, the changing concept, the self-identification with Buddha; he also brought out the marked difference between Hinduism and Buddhism. On the other hand, Mr. Turner, of the American Embassy in Tokyo, emphasized the sectarianism, the animism of that religion.

To me the followers of Buddha appeared as perfectly admirable people, who were not anxious to impose their concepts upon others but were willing, indeed anxious, to listen to other ideologies and religious beliefs. The priests and many of the professors in Japan had emphasized that Christianity was no answer to their problem, even to the confused state of mind of many. They would have liked to look to science for the answer, but they did not quite know how. A strong personality is bound to come, who will sweep everyone before him, just as during the period of decadence of Greek civilization and the Roman empire, little Judea, politically destroyed, nevertheless pointed the way to "salvation" to the very people who were responsible for its national destruction. No wonder that many Japanese find an outlet for their religious feelings in Shintoism, which is a mode of life and family reverence; in their Noh and Kabuki plays, which provide an escape from their present environment; and in their reverence of outstanding personalities, as exemplified by the Robert Koch shrine in the Kitazato Institute grounds.

We were sorry to leave the pleasant atmosphere of Hawaii. But we had to go home. At the San Francisco airport, we were met by

representatives of Mount Zion Hospital, where I was to deliver an address. As we had little time to go out of the city, a few of our friends, colleagues, and former students came over from Palo Alto, Berkeley, and Davis. There was a big dinner and reception the night before my scheduled lecture. Dr. Bloch, who shared this year's Nobel Prize in Physics, spoke of Nobel's work, the Nobel Foundation, and its prizes. Dr. Hinshaw said a few words. I spoke for about ten minutes, largely repeating what I had said in Stockholm in accepting the Nobel Prize. We ended the evening by joining some of our friends from Berkeley in the international settlement.

On the flight home, because of heavy fog the plane had to land at La Guardia instead of Idlewild. Thus we missed making a complete circuit of the globe by five or six miles, the distance between Idlewild and La Guardia airports.

Practical Aspects of Scientific Endeavors

AS I reached my sixty-sixth birthday, it became desirable to look back and measure or analyze my successes and my failures. I had to finish all my recollections of the past and begin to look again to the future, the future of the microbes to which I devoted my life. This could best be done by re-examining some of the phases of my past activities, notably certain practical and personal problems, which tended to overlap my years of scientific activities.

Since I was ten years old, I have learned to render help to others in the solution of their problems. At the tender age of ten, I became a teacher. I taught those who knew less, who had fewer opportunities, or who were less capable of assimilating knowledge.

As I grew older and left the town where I was born and where I spent my childhood and youth, and later my native country, my teaching experience was of great help to me. It contributed materially to the limited funds available for my living expenses and for furthering my own education. Even as a student in college, I taught other immigrants the principles of the English language and prepared some of them for college entrance. Fortunately, opportunities to make use of my early scientific and practical agricultural training also arose before very long. Thus as an assistant to the college botanist, I made use of and extended further my knowledge of plants, their nutrition,

their reproduction, and their life habits, first merely as an apprentice, and later in an independent capacity.

The opportunity to render advice as a full-fledged scientist presented itself during the end of my year's graduate work at the college, in 1916. We were then in the midst of a great war. Although the United States, of which I had become a full citizen only a few months before, was not yet participating as an equal belligerent, it rendered considerable help to the allied cause in certain indirect ways. A company producing explosives for one of the allied countries suddenly began to experience certain failures in its manufacturing process. Fungi were blamed for causing partial destruction of some of the materials used in the manufacturing process. Since no one at the company in question knew anything about microorganisms, the chemist in charge called upon the college to send over someone who knew how to work with such organisms. Fortunately for me, it was summer and the plant pathologist who was then the person most familiar with fungi was away. I was asked whether I would be willing to tackle the job, since as a result of my work on the soil fungi and actinomycetes, I was now well versed in these groups of organisms.

I spent several days at the industrial plant and made various tests for the presence and distribution of fungi. I found these organisms in great abundance at certain stages in the manufacturing process and suggested a method of treatment to prevent their further development. Whether this recommendation was followed, and, if so, with what success, was never reported to me because of the secrecy of the operations. I succeeded, however, in earning a small sum which helped pay our travel expenses to the Pacific Coast, where I was to complete my graduate training at the University of California.

Upon arrival in California, we found ourselves again with very limited funds. The fellowship that I received from the university was quite inadequate for my young bride and me, even for a very modest way of life. Now again, I was forced to look for a supplementary source of income. I did some tutoring in the field of chemistry. I carried out various chemical and biological analyses for colleagues.

My experience at the laboratories of a biological organization offered me further ideal opportunities to acquire first-hand knowledge of the subject of immunotherapy, that is, the utilization of biological

agents produced by the human or animal body to combat infectious diseases. Now I learned to appreciate fully the practical significance of my theoretical knowledge. I was especially impressed by such problems as the production of antitoxin against diphtheria (although this antitoxin was discovered by Von Bering in 1891, it was still unavailable in Priluka six years later, when my two-year-old sister died from this disease), of vaccines against typhoid, and of various serums against a variety of other infectious diseases. During my year's work in these laboratories, and it was the significant year 1917-1918, when the United States entered the World War and when American biological and chemical industries started on their path of great development, I was called upon to apply my knowledge of bacteriology and biochemistry to the solution of a variety of practical problems.

Thus my scientific training in microbiology and biochemistry at the university was supplemented with practical experiences in the utilization and application of the knowledge thus gained. It gave me an opportunity to visualize the applied and practical phases and to see that theoretical knowledge would sooner or later find application to human economy and human welfare.

Before very long, I had an opportunity to learn this on a far greater scale and in a far more responsible manner than I ever dreamed of before.

Upon the completion of my work at the University of California, I gave up my industrial position to return to Rutgers, where I was to engage again in the study of the microbiology of the soil. Unfortunately for my hopes of continuing in my beloved field of research uninterruptedly, but fortunately for my practical experience, I was again looking for a supplementary source of income. Because of the financial straits in which I found myself upon returning to New Jersey, because of the low salary that I was receiving (fifteen hundred dollars a year) and the rising cost of living at the end of the first World War, it was absolutely necessary for me to obtain some additional income if I were to continue with my research and teaching career.

This was now highly feasible and fairly simple, since the chemical and biological industries in America were undergoing a tremendous development as a result of the war. Among the many tempting offers that soon came to me was one particularly to my liking. This came

from the Takamine Laboratories engaged in the manufacture of enzymes produced by fungi and of a certain chemical agent used in the treatment of a serious infectious disease. Here I first came in contact with a field of science which was to influence my future career, although at a much later date.

The position in question was concerned primarily with the toxicological studies on salvarsan (606), the first synthetic compound that had been introduced as a chemotherapeutic agent just a few years previously by Ehrlich in Germany against the destructive disease of syphilis. This substance gained great importance during and immediately following the war because of the great spread of this disease by the returning soldiers. Fortunately salvarsan proved to be a very effective, although not ideal, weapon in controlling the disease. Large-scale production of the drug became a problem of great importance. Ehrlich's collaborator, Hata, who assisted him in establishing the therapeutic value of salvarsan, started, upon return to his native Japan, a small plant for its manufacture. The Japanese company induced Dr. Takamine to undertake the production of the drug in the United States. For this purpose, a group of trained chemists was brought over from Japan and a new industry was started. However, salvarsan was frequently toxic to the human body, and, before it could be offered for practical use, its low toxicity to experimental animals had to be determined in accordance with government regulations. I was given the job of measuring the toxicologic properties and of standardizing its effective, nontoxic limits.

Here was an unexpectedly new problem, quite distinct from what I had done previously. It opened up new horizons, bearing upon the possibility of combating diseases by means of chemical compounds, the whole new field of chemotherapy. This approach was different from my previous practical experience, which dealt also with problems of disease control by the use of bacterial products, what might be designated as biotherapy rather than chemotherapy.

Since I first learned, late in 1910, of Ehrlich's great discovery, I was fascinated by the idea of finding chemical substances which would be effective in the treatment of infectious diseases of man and animals. Now, only eight years later, I was called upon to participate actively in the production of this important chemical compound for

saving human lives. Little did I dream even then that the very soil microbes with which I was dealing in my own research work and to which I was to devote a quarter of a century of study, would themselves possess the capacity to produce such life-saving substances. Little did I dream in 1918 that the "magic bullet" of Ehrlich that fascinated me so much would later be replaced and much further extended by the "wonder drugs" produced by microbes largely derived from the soil. Still less did I dream at that time that one such drug, one of the most important of them all, would come from my own laboratory, from a culture of one of the actinomycetes that I had already isolated from the soil three years previously, at the very beginning of my own work on the soil microbes.

Metchnikoff had shown that the cholera germ planted on culture plates inoculated with various other bacteria was either benefited or injured by such organisms. He followed these observations by similar experiments in the animal body, but his results were rather inconclusive. The nature of the intestinal bacterial flora of man and the methods of modifying it intrigued him greatly. He emphasized that "the intestinal flora is the principal cause of the too-short duration of our life, which flickers out before having reached the goal, and which science must set to work to correct. It will succeed in doing so, and it is hoped that the opening century will witness the solution of this great problem." In speaking of the insane war that engulfed the human race in 1914, he said further: "Let those who will have preserved the combative instinct direct it toward a struggle, not against human beings, but against the innumerable microbes, visible or invisible, which threaten us on all sides and prevent us from accomplishing the normal and complete cycle of our existence."

These words of the great Russian bacteriologist made me think. They were fascinating, but they were still rather vague. Even the experience that I gained from my work on salvarsan was not sufficient to touch off the spark. Two more decades were to elapse, however, before the synthesis of new ideas could be brought about.

When I left Takamine Laboratories after a little more than two years, to devote my full time to the soil microbes, ideas for practical application of microbial activities, both for disease control and for industrial manufacture, were deeply implanted in my mind. No mat-

ter what I was to do now, I would think of what it was all about and how such studies could be used to the benefit of mankind.

Three years later, in 1923, I was approached by a representative of an organization manufacturing textile products for advice on a process of making diastatic enzymes by fungi. I had some theoretical and practical experience along these lines, since my doctor's thesis dealt with this subject. Also during the Takamine interval, I had done some research work in this field. With the approval of the director, I undertook to help the organization. Once a week, I would spend part of an afternoon at a plant in northern New Jersey, discussing with the chemists of the company their problems of growing fungi on a large scale and of obtaining the most satisfactory yields of enzymes. I recommended to them one of my former students for the position of bacteriologist, and, before long, the process was working smoothly. Here again, I had an opportunity to translate my theoretical knowledge into practical applications.

No sooner did this connection end, when my task was considered as having been accomplished, than I was invited by another company located in Philadelphia and also engaged in the manufacture of enzymes to help them in the development of another type of enzyme, this time one that would attack proteins. This was about the time (1926) that insulin was discovered, and there was danger that all slaughterhouse-produced pancreases would be used for that purpose. The company in question used these glands for making proteolytic enzymes. It was decided, therefore, to look for microbes which could accomplish the desired purpose. Here again, I would go once a month to the plant and discuss with the chemists and bacteriologists of the company the problem of growing fungi, of developing suitable media, and of obtaining maximum enzyme yields. This time again, I recommended one of my students for the position of microbiologist. He carried out the actual manipulations at the plant. My services proved useful to the company, and my connection lasted more than a dozen years. Long after my actual services ceased to be essential, I continued to receive my monthly retainer. This association also proved to be useful to my research laboratory, since the company made grants and established fellowships for more extended research in the field of bacterial fermentations.

In two instances, however, the company failed to follow my advice, thereby delaying for several years progress in the field of antibiotics. When Dubos demonstrated in 1932 that certain soil bacilli could be made to produce a type of enzyme which had the capacity to dissolve the capsule of certain bacterial pathogens, thereby rendering these germs subject to rapid destruction in the human body, I urged the company to undertake the practical exploitation of the process. I suggested that this approach was bound to lead to other procedures for developing chemical substances that had the capacity to attack bacterial pathogens. The company refused to accept my advice, since they did not want to be brought into the field of medicine and disease. Perhaps I myself was not yet fully convinced of the tremendous potentialities of that field. Otherwise I might have encouraged others more interested in such problems to undertake them.

I was not yet ready to change my work in microbiology from the study of the soil to that of disease. This was well borne out by the study of the fate of tubercle bacilli in the soil. As already discussed previously, we found that these bacteria do not remain long in the soil, but are rapidly destroyed there, and that certain soil microbes appeared to play a role in the destruction of the pathogenic microbes. Still I was not fully prepared, either mentally or experimentally, to make practical use of this observation and carry the problem to a practical conclusion. When the poultry pathologist of the Experiment Station brought me, about that time, a culture of the tubercle organism which became infected with a green mold, the latter being responsible for the destruction of the pathogen, I was not moved to jump to the logical conclusion and direct my efforts accordingly. The time had not yet come. However, these facts were gradually accumulating in the back of my mind, until the time would be ripe when I would be prepared to attack directly the problem of the effect of soil microbes upon pathogenic organisms.

Meanwhile, my industrial experience continued to grow as new practical or applied problems were brought to my attention. I no longer had to look for them. They came to me. I accepted them only when I felt that I could render a distinct service, only when I knew what the problem was and what my efforts might bring forth. I never promised anything that I was not sure of being able to accomplish. I have re-

fused time and again to have any connection with a company, the motives of which did not appear to be most ethical, in spite of the remunerations offered.

One interesting experience might suffice. When, during the early thirties, I was actively engaged in unraveling the riddle of soil humus, its origin, nature, functions, I was called upon to make some practical applications even of this subject, which appeared so abstract and so devoid of industrial potentialities. One fine summer day, there entered into my tiny office at the Experiment Station a young man representing a large lead concern engaged in the manufacture of automobile batteries. After introducing himself, he began: "I was recommended to you as one who knows all about humus." My reply was: "Hardly all about it; but what has humus to do with an automobile battery?" He then proceeded to tell me a long and complicated story, leading up to the fact that the chemists of his company had discovered recently that addition of small amounts of humus to the negative plate greatly prolonged the life of the battery. Having made this discovery, which was expected to be of great importance to them, they found that another company, actually their major competitor, had already applied for a patent on this process. The problem was, therefore, to find out whether "humus and humus are always the same thing." This was the question that he presented to me. I accepted the challenge and undertook to help. At my suggestion, one of the chemists of the company came to my laboratory and demonstrated the details of the process to me. Having familiarized myself with the two processes, I came to the conclusion that they employed different types of materials, which gave them different types of humus. A further study of this process convinced me that much more could be accomplished by further purification and isolation of the humic material. These studies resulted in a new patent application which threatened the patent of the other company. The result was that both companies came to a settlement, thus avoiding a costly lawsuit.

There were other problems not so striking, but just as interesting, in which I was called upon to render advice or to help make a decision. I need only mention problems involved in the utilization of marine algae for the preparation of industrially valuable products, and problems of destruction of cellulosic materials. Often consultations of this

nature were followed by a suitable compensation; at other times, a good dinner and expenses were my only remuneration. But this really did not matter so long as satisfactory results were obtained. My scientific work greatly benefited from my increased appreciation of the practical potentialities of microbiological and allied problems. I always saw to it that nothing interfered with this work. I used the extra compensation to advantage by making additional scientific trips to Europe, by subscribing to some extra scientific journals and books, and, occasionally, by obtaining grants or fellowships for the laboratory. Having left financial worries behind me, I could now devote not only my days but also my nights to the writing of another scientific book, to the planning of additional scientific experiments.

These practical problems fully prepared me for the two most significant ones that were soon to require my full attention. One was in the field of antibiotics, especially their practical applications in the control of disease, and the other involved my services rendered to the government during the Second World War, in connection with the deterioration of service materials under tropical conditions.

The story of my entrance into the field of antibiotics and the subsequent developments in this connection has already been told. Two aspects, the industrial and medical utilization of these products, have not been emphasized sufficiently. Soon after I decided, in 1939, to change the investigations carried out largely in my laboratories from the study of soil microbes to the problem of antibiotics, I was approached by representatives of Merck & Co., with the following proposal: "We are interested in the practical developments in this field. You will need chemical assistance, which we are prepared to give you. You will need animal experimentation, which we are prepared to do for you. You will need large-scale production, which we can take care of. In exchange for all that, we would like to be given an opportunity to examine the chemical substances that you are bound to isolate. If we should decide to manufacture any of them, we would want an exclusive license. We are prepared to apply for patents in your name and to enter into a contract with the university and pay a royalty of two and a half per cent on any manufactured products using processes that you and your assistants will have developed." Thus began one of the most fruitful connections ever entered into between a uni-

versity and an industrial organization. It was to result in great benefit to both. Our relations with Merck actually began as far back as 1938, when that company first established a fellowship in our laboratory for the study of citric and fumaric acid production by submerged fermentations.

Meanwhile, the relations established with Merck in our collaboration in the field of antibiotics proved to be of inestimable value to us in making rapid progress in the field. The company lived up to all its promises. When the antibiotic actinomycin was first isolated in our laboratories in 1940, the Merck chemists spent much time in crystallizing it and establishing its chemical structure; their pharmacologists determined its toxicity in animals; and the manufacturing plant helped us in developing larger-scale production than was possible with the limited facilities of ordinary small-flask operations. The same held true of the other antibiotics that we isolated later.

On the basis of the contract with the university, this company applied in our name for patents, first for actinomycin, then streptothricin, then streptomycin. My name and that of one of my students who happened to be working with me on the particular problem was attached to the patents. Assignments by us were expected and obtained. Royalties to the university would be paid when and if these processes became practicable.

As discussed previously, the discovery of the potentialities of streptomycin made me reconsider this exclusive contract. We were dealing with a public-health problem, which was bound to arouse considerable criticism of, first, the mere fact that we had applied for patents and, second, that we turned them over to a single company for exploitation. The only thing that would save us was to abrogate the contract and make the processes available to all industrial concerns that would care to make use of them. When we approached Merck with the request that the company cancel the exclusive-license agreement and make the processes available to all others interested in them, the company most generously agreed to our requests. We were helped by the fact that this took place at a time when this country was still at war and by the fact that Merck, as well, tried to avoid the criticisms to which the control of public-health patents is subject.

Thus the university found itself in possession of valuable patents.

Unfortunately, there was still no proper organization to take care of arrangements for suitable contracts at home, of patent applications abroad (none were ever obtained in Europe), and of proper assignments. At first I handled all matters personally. Thus I became at once the discoverer, the planner and organizer, and the legal negotiator with many companies interested in the manufacture of streptothricin and streptomycin. It placed a terrific burden and responsibility upon me. If one adds to this the tremendous demand on my time during the period subsequent to the discovery of streptomycin for lectures, special papers, and trips abroad, one can understand that certain phases of these arrangements were not given proper consideration. It took nearly a year before the Rutgers Research and Endowment Foundation was organized and was in a position to handle all legal matters. Meanwhile, certain errors (neglect in making foreign patent applications, lack of proper assignments to the foundation, imperfections in some of the contracts) had already been committed. It took several years of effort and considerable unpleasantness to untangle some of these.

Once the foundation was organized, the trustees considered my own relations with them. First, it was essential that my services be available to them for a considerable period and that past and future services in the discovery of the antibiotics, in planning their practical applications, and in organizing their proper exploitation be given some definite recognition. A contract was drawn up between me and the foundation whereby for five years (later this contract was modified to read for my whole future activities) I was to turn over to the foundation all patentable inventions and to serve as director and adviser for the foundation in all its activities.

My intimate relations with an important industrial organization were of the greatest benefit to my scientific work. I was able to visualize in broad perspective the laboratory problems carried out on a small scale. I was able to take advice and help from outstanding chemists. The pharmacologists were always ready to test and evaluate, by animal experimentation, any new antibiotic that was isolated in my laboratory. The company as a whole was ready to consider the practical aspects of any problem. The services I rendered to the company and the realization of the practical potentialities of my own work could

never have attained the magnitude it did without my previous experiences with the other industrial organizations, notably those manufacturing the antibacterial biologicals, enzymes, and salvarsan.

Thus, as I look back over my many years of scientific and practical work, I realize how both have dovetailed, in spite of the fact that the former dealt with such almost purely abstract subjects as the microbiological population of the soil, notably the fungi and actinomycetes, the problems of humus, the oxidation of sulfur by bacteria, and a variety of others; the latter, however, were immensely practical, dealing with such problems as desizing of textiles, clarification of fruit juices, degumming of leather, production of lactic and citric acids, the making of automobile batteries, and finally the utilization of antibiotics for disease control. But how much they had in common! How much one depended on another, one led to another! Just as my lectures depended on my scientific work, just as the latter influenced my writing of various books, so all these were closely integrated and interrelated.

I spent, during the war years, many a day in Washington attending meetings of committees of one sort or another, of one military branch or another, visiting installations, examining service materials. I spent many a night in preparing handbooks for practical use and in writing voluminous reports. Fortunately, a number of active people were soon brought into this work, so that I could gradually withdraw to devote my time to other problems.

I thus gave much of my time and energy to contribute my share toward winning the war and toward alleviating human suffering. Most important of all, this helped me to forget the world catastrophe and the resulting misery that brought so much destruction to my friends and close ones, both in the land of my birth and in my adopted land, that threatened the life of my only son here and that extinguished the life of the only son of my closest friend, Peisi, over there. To work, to forget! Let others benefit from it! When so many spend their energy to destroy, let there remain the few who would help to rebuild, to save human lives, and extend hope for a better future life.

Having thus been at least partly responsible for the discovery of streptomycin, a drug which opened the road toward the final conquest of tuberculosis, and, especially, having received the Nobel Prize for

this work, I became a handy subject for adulation and newspaper publicity. Apparently, the world expected that I could pull new "wonder drugs" out of the hat, as if by magic.

I well recall an address I delivered a year or two before the award of the Nobel Prize, at a symposium held at Wayne University in Detroit. In listing diseases that were still not subject to drug therapy, I expressed the hope that antibiotics or similar substances might be found which would do for these diseases what the sulfa drugs and penicillin were doing for pneumonia and numerous other diseases caused by gram-positive bacteria, and what streptomycin and other antibiotics were doing for diseases caused by gram-negative bacteria and tuberculosis. The chairman of the meeting then declared enthusiastically, "If anybody can do it, it will be Dr. Waksman who will do it."

But an even more embarrassing incident occurred at the Sixth International Congress for Microbiology, held in September, 1953, in Rome. I was invited by the organizers of the congress to preside over a symposium dealing with the actinomycetes and their economic importance, and to present certain introductory remarks. I accepted this invitation and prepared a summary dealing with the progress made in recent years in our knowledge and utilization of these organisms. Naturally, I emphasized the progress made in the field of antibiotics as a whole and the role that the actinomycetes had played in this development. I said, among other things, that the most important antibiotics that had found practical application in therapy, following that of penicillin, were produced by actinomycetes. I also said that although some one hundred and five antibiotics had been isolated, either in pure or in crude form, from cultures of actinomycetes, only about ten had so far found practical application. Most of the others had been discarded either because they were too toxic or not active enough. I expressed the hope that sooner or later many of those that had been discarded temporarily would be studied further: perhaps some uses might be found for them.

I illustrated this possibility with actinomycin, the first antibiotic that my assistant Woodruff and I had isolated in 1940, at the very beginning of our work on antibiotics. It had been set aside because its toxicity was so great that one milligram would kill a four- or five-pound

chicken. Another of my students, Harry Robinson, who had done the pharmacological work on actinomycin, had observed that the antibiotic was particularly effective against certain cells and organs, such as the spleen. We would frequently discuss the desirability of making further studies with actinomycin, which, as my first-born, was especially important to me. During these years, many other studies had been carried out on actinomycin, notably in Germany and in England. Several new forms had been isolated. In recent studies, in Germany, one of these forms, actinomycin C, had been found to have a depressive effect upon certain abnormal (cancerous) cells, especially those of the lymphatic system, thus benefiting the patients. The conclusion had been reached that actinomycin C had a possible therapeutic effect in cases of malignant diseases of the lymphatic system. When one hundred and sixty patients had been treated with very small amounts of this compound, favorable effects had been observed, especially in the treatment of lymphatic tumor, or conditions known as Hodgkin's disease.

At the close of my address, an Italian journalist immediately sent out a news story that appeared under the heading, "Waksman Discovers Antibiotic Active against Cancer." This story was reprinted with all sorts of modifications in various newspapers throughout the world. I was flooded with cables, telephone calls, and letters from cancer sufferers. In spite of my denial that I had ever claimed any such thing and that I had merely made incidental reference to the fact that various discarded antibiotics might be restudied to determine certain properties not recognized previously, the newspapers continued to play up the story. Most of them buried my denial somewhere where it would not be noticed readily.

One fortunate outcome was that this episode served to arouse greater interest in actinomycin among scientists and clinicians, and in stimulating further search for antibiotics active against malignant cells.

As I Look Back

Little is popularly known about the thoughts and doings of men who spend their lives in laboratories trying to discover new facts through carefully devised experiments. A favorable condition for productivity in research is variety of experience, both one's own experience and that which may be derived through observation of others who are at work on different problems. Especially is this important during the early years of education and discipline. As an investigator continues in his career, accident will present him with unpredicted opportunities for research, perhaps in quite new directions.—Walter B. Cannon.

AS I look back to a lifetime of research, devoted to the study of the microbes, their occurrence in the soil, in the sea, and all around us, their manifold activities, and the part they play in the cycle of life in nature, I might well conclude that it was planned that way. But was it? Had the environment been different, had the schools attended, the opportunities presented, been different, my whole life might have been directed along different channels.

Had I looked for a job in a factory or in an office to avoid the hard work in a shop for my young bride at the beginning of my career (1915), I might never have started on a research program. Had I been satisfied with my industrial positions at Cutter's and at Takamine's, perhaps I would have gone on with some form of manufacturing and not gone back to the study of the soil, the microbes of the soil, their hidden secrets and potentialities. Perhaps so, although I doubt it. Sooner or later, in one way or another, I would have returned to them.

There was a certain driving force that gave me no rest until I reached the goal. But what was the goal? What kind of a force? And why? When I first learned about the microbes, I almost said to myself: "To these you shall devote your life." I was curious about them. I was attracted to them. I wanted to know more. When each spade of soil uncovered new forms of life, when each new organism pointed to thousands of others, I was encouraged by forces beyond my control, as if there were somebody at my back urging me on and on: Look again, search for other organisms. Perhaps there are certain principles, certain universal laws which will become clarified, as you dig again, as you repeat one experiment after another.

Fortunately, my first research experience was under a highly gifted man (Dr. J. G. Lipman) who knew what there was known of the soil microbes at that time, and who was honest enough to say that he did not know much. When I presented certain pointed questions to him, he would say: "For these there is no answer. You will have to go and learn new methods, develop new techniques; you need to know more chemistry, more physics, more biology, more of a lot of things."

When I presented my questions to the brilliant biochemist, under whom I did my graduate work (Dr. T. B. Robertson), about the potential roles of microbes and methods of studying them, he would also answer: "Study some more, do not forget your chemistry and especially your mathematics. These will help in elucidating a mass of confusing data, perhaps point to universal laws." Then, there was always in the back of my mind what my old friend and mentor (Dr. B. H. Halsted) used to emphasize: "Stop and think; do not be attracted by popular ideologies, by glittering superficialities; try to find your own way. If you are attracted by the microbes, study them, think of them, find your way among them."

As I surveyed the literature of the subject of soil microbes, I felt discouraged. There were masses of data, some full of meaning, some conflicting, and many in a state of chaos. How could one bring order to this chaotic state? Some data must be re-evaluated, others supplemented, still others rechecked and reinterpreted. Was I to do it? Was I to clean the Aegean stables? Who was to fill them again? The task was immense, the facilities limited. As I began to publish one paper

after another, they attracted little attention. An occasional encouraging voice was heard, and frequent sneers ("So what?"), but mostly mine was the voice in the wilderness. I labored alone, or with the help of a few assistants. Some understood, others were not much interested. Some went along to continue what little I had begun, others forgot everything as soon as they were through, and one or two even went off sneering ("Who does he think he is, anyway?").

I returned from Europe in 1924 refreshed and encouraged, full of ideas, anxious to start again. But first of all, I must reinterpret, re-evaluate the accumulated facts and problems. I must write a book. Not just a mere compilation, but an interpretation. I must give the younger minds something to feed upon. I must prevent them from becoming discouraged and, worse, from falling into pitfalls. I must arouse their interest. I must point a way to them.

And thus I spent my days and my nights reading, digesting, separating the grain from the chaff, and came forth in 1927 with a large volume on the soil microbes. It comprised the "Principles" on which to build further. This volume was acclaimed by some, criticized by a few, and looked at with curiosity by most. After all, what is all this fuss about? Is it worth all the trouble? And who was I to get so excited about it?

I have often been asked how it came about that I wrote so many books. Where did I even find time to do it, in addition to my scientific work? My answer would be: "I experimented on my students." In giving my lectures, regularly, to groups of graduate students, young, eager minds, most of them, I neither had the interest nor the ability to repeat what one could easily find in written books. Then, I did not agree with a good deal that had been written. Why waste my time and theirs, when a subject could be read in a book? Then, I wanted to lecture about what could not be found in any book, at least not the kinds of things I wanted my students to know.

When I came back from my long and exhausting European journey, full of enthusiasm and ideas, I knew that I had the answer to many of the puzzling questions. I knew that by establishing its scientific value and practical potentialities I would gradually win recognition for the subject about which there was mostly scorn in some quarters, notably in my own country ("What has soil microbiology, aside

from the legumes, ever contributed to modifying soil practice and to improving the lot of the practical farmer?"').

Following my books on soil microbiology and humus, I devoted my lectures to the associative and antagonistic effects of the microorganisms, to the formation of antibiotic substances, to their effect upon bacteria, and finally—and all this was particularly new—to their potential use in the control of disease.

Again, the same story repeated itself. Within five years, my lectures took the shape of a new book, *Microbial Antagonisms and Antibiotic Substances*. This book was written in the midst of the war (1943–1944). The Commonwealth Fund, a philanthropic organization that had been supporting with a grant my scientific work on antibiotics, became interested in the publication of this book. The subject grew so rapidly that I hardly felt justified in taking the necessary time to prepare other editions. Numerous other books began to appear on this subject, so that I felt that my own task had been accomplished.

The tremendous progress made in the use of streptomycin in the treatment of human and animal diseases necessitated my compilation, with the assistance of other pioneers in the field, of a comprehensive treatise on this antibiotic. This time the book (*Streptomycin*) did not result from my lectures, but from the actual investigations by numerous microbiologists, chemists, pharmacologists, and clinicians.

These and certain other books that I have written, alone or with others, are ample evidence of my share in contributing to the development of the science of microbiology to which I have devoted my life. But were my students satisfied? I believe they were. By concentrating my lectures on some special phase of the subject, I could inspire them to become interested in the field as a whole. I could give them the benefit not only of what little I possessed of theoretical knowledge, but also of the many actual and potential practical applications of the subject. I never missed an opportunity to tell them a suitable story to illustrate a particular event. Although I never claimed to be a storyteller, and certainly I could never begin to approach the masterly handling of the art of storytelling of my gifted teacher Jacob Lipman, I could always see a ridiculous approach to a subject and, by calling attention to it, help my students avoid potential pitfalls.

Was I successful in this manner of teaching microbiology? This is hard to tell. No doubt some students greatly benefited from this method of approach. Others fell by the wayside, never having learned to familiarize themselves with the broader aspects of microbiology and leaving our laboratory with a scant familiarity even of that phase of the subject which I tried to teach them. One of them, who took his course with me during the beginning of my active interest in antibiotics, actually said, to my eternal disgust: "I did not learn any microbiology at all; all I learned was antibiotics." And this was at a time when a knowledge of antibiotics was worth far more than "its weight in gold," from both a theoretical and a purely practical point of view, since the many pharmaceutical houses were willing to pay any price for the services of one trained in this field of knowledge. But such is human nature. What was I going to do about it? Should I teach the few potential leaders? Or should I popularize the subject in order to make everybody learn something, and let the gifted ones pick up by themselves what they could. I presume that this sort of problem faces every teacher.

As to my attitude to science, I may refer again to the microbiologists whom I have admired most, Winogradsky and Beijerinck. The former was a classicist, according to Oswald's classification. When, soon after he settled in a new position at the Institut Pasteur in Paris, I asked him to accept one of my students for a year's postdoctorate work, his reply was simple: "Why should he come here? What I can do, he cannot do; what he can do, I am not interested in." Although his scientific career was long and fruitful, he did not leave one worthy successor. On the other hand, Beijerinck was a true romanticist. He established a great school of microbiology at Delft, successfully continued by his worthy successor Kluyver; he initiated many investigations in microbiology, which were continued by others in all parts of the world.

Many investigators, largely those in the United States and more especially those in France, have done most of their scientific work themselves. When students come to their laboratory, they are assigned special problems and are soon forgotten. Rarely does a student's investigation reach maturity during his apprenticeship; more often, it

is a basis for a future scientific career. No wonder it is often said that a scientist carries his doctor's problem always with him.

There is another group of investigators, largely in Germany and to some extent in the United States, who assign to their students certain small phases of their own major problem; often one student does not know what the other does or what relation his small problem has to the problem as a whole. Fortunate is the student who is attached to a large problem, no matter how small his own participation in it may be; he becomes identified with a school, and throughout the rest of his life, he remains a member of that school.

In my own work, I tended toward the second type, for several reasons. My position was always that of an investigator, as a research member of the Experiment Station staff, rather than that of a teacher at the university. I held an appointment at the university, however, and was required to offer a course to graduate students. These students had to do their thesis work with me. The fact that the station allowed me limited funds for an assistant, and such funds could be used more advantageously to help support one or two graduate students, who thus served as part-time assistants, facilitated matters. Further, various industrial organizations were interested in some of our research work and offered grants in the form of fellowships, which served to support other students and visiting investigators. The latter were also helped considerably by various foundations. Another contributing reason was that there were only very few soil microbiologists in the country, and still fewer were interested in the soil microbiological population as a whole and the mutual interrelationships of its constituent members. Because of this and because of my personal interest in those who worked with me, who helped me in the solution of my problems, and who often inspired me by their curiosity and desire for truth, I identified myself with them. It thus came about that many of those who worked under me were inspired to continue with a research career. I hoped to contribute in this manner to a much greater advance of our understanding of certain natural processes than would have been possible had I worked alone.

I can truthfully say that I owe much to my professors, that I owe more to my colleagues, but that I owe most to my students, of whom I

had a brilliant array. What greater joy can there be, in discussing certain scientific accomplishments, than to point also to these former students and say: "I have helped in shaping the minds of these so that they could become what they are today."

Unfortunately, a few, to be sure only very few, have failed—failed in my hopes, failed in my expectations. Let these be forgotten, since they simply serve to illustrate that human society is made up of a great variety of individuals who range from the constructive, idealistic, and progressive to the destructive, the drones, and even the plain parasites.

As I look back upon this body of some two hundred or more men and women who came to my laboratory from every corner of the earth, I see humanity in microcosm. I tried to teach these students a concept in a field of science. I tried to point out to them new paths to follow, new syntheses to make. Some have taken full advantage of their opportunities and have gone beyond all possible dreams. Others have fallen by the wayside. Many have carried on to the best of their ability; a few have blazed new trails, have made some startling contributions, and have thus fully justified my investment in their training, if not my hopes and desires.

This may be the place to dwell a bit upon the life and work of my own teacher, Dr. Jacob G. Lipman. As a leader in research, he believed in finding the right man for the job, then giving him full freedom to do the job the way he saw fit. He always encouraged young investigators and was anxious to develop the research spirit in the best possible tradition, without overlooking the potential practical benefits to farmers and to society as a whole. Having given up his own research career at an early date and having assumed administrative duties at the Experiment Station and College of Agriculture, he was always dreaming of the possibility of returning to the laboratory to re-enter the field of research in soil microbiology, so dear to his heart.

Dr. Lipman left an indelible effect upon soil microbiology in the United States, in spite of the very brief period during which he was actively engaged in this field. This was due primarily to three factors: 1. The problems with which he was concerned, namely, the decomposition of organic residues in the soil, the liberation of ammonia, the fixation of nitrogen and nitrification, oxidation of sulfur, were all

fundamental to a proper understanding of soil fertility and the cycle of life in nature. 2. He had a limited number of students, whom he encouraged to follow in his footsteps. 3. He was a brilliant speaker and always emphasized, in his public addresses, the importance of soil fertility to the progress of human society.

Dr. Lipman frequently envisioned an institute where men from all over the world would gather to study the soil, its physical and chemical properties, biological activities, and role in human economy. Unfortunately, he was too busy with administrative duties to spare time for the establishment of such an institute, which would have been a monument to his scientific spirit, his social philosophy, and his humanitarian interests. He devoted much of his time to preaching to farmers the importance of research for more efficient utilization of soils, for better growth of crops, for improved yields of milk and eggs. But he had to devote too much of his energy to convincing the state legislature that improved soils, greater crops, better farm stock, would serve not only in improving the well-being of the farmer, but would also contribute to the welfare of society as a whole. In the end, he had little time to take an active part in the research program of his own department, much less to fulfill his long-cherished dream of building an institute. He died altogether too soon, at the age of sixty-three, just on the eve of a great revolution in microbiology, when the soil investigator was able to contribute so much to the advance of medical science and practice and in which some of his own students, in the laboratory that he had planned and built, took such a decisive part.

Dr. Lipman was the son of a poor immigrant family, which came to the United States in the early 1880's. His father was a pioneer farmer, who, together with a group of other idealists, tried to build up a school for the training of young immigrant boys for the profession of farming. Young Lipman himself was graduated from that school in 1894, then came to Rutgers for a college education in agriculture. He had a hard time during his college years, but managed, by milking cows and doing other odd jobs, to work his way through. Upon graduation, he was assigned a project on nitrification that was at that time receiving universal attention. Lipman felt that for this work he needed further training in bacteriology. He went to Cornell University, where he enrolled for graduate work in bacteriology. Upon

his return to Rutgers in 1901, he decided to devote himself to the study of soil bacteria.

When I first came to see him in the fall of 1910, soon after my arrival in this country, he was on the point of giving up active research and taking over his administrative duties. He managed for another decade, however, to teach the regular course of bacteriology at Rutgers and to continue to direct certain investigations through some of his assistants and students.

I may truthfully say that I followed in his footsteps and tried to take up the study of microbes where he left off. But my own interests were not primarily in practical agriculture. The microbes themselves and what they are able to do attracted me most; what bearing they might have on soil processes was of only secondary concern. Dr. Lipman's major interest, on the other hand, was the soil and its utilization for crop production; the microbes interested him only to the extent that they helped explain these complex processes. His sincere appreciation of the importance of fundamental science in the solution of practical economic problems can best be illustrated by the following story.

Upon my graduation from college in 1915, I was inclined to go to a larger university, where there was greater opportunity than at Rutgers for selecting courses in the fundamental sciences. With this in view, I applied for a fellowship in the Soils Department of the University of Illinois. The head of that department, a well-known soil scientist, Dr. C. Hopkins, refused to grant such a fellowship to me on the grounds that he "could not waste public funds by supporting a student who did not come from a farm and, therefore, could hardly be expected to make an important contribution to practical or even scientific farming." When I showed that letter to Dr. Lipman, he smiled and said: "How little he appreciates that help to the farmers must come from the fundamental sciences and from those trained in them." He then added: "You had better stay another year here. It will give you a chance to find out something about the soil microbes; they will have to listen to you then." That I followed his advice, I have never had occasion to regret.

Dr. Lipman was a gentle person. He was anxious to do all he could for those who came to him for help. I often thought that it was really not he who selected his associates, but they who selected him,

for he could never refuse, unless it was physically impossible to accept them, or funds were lacking, and he could find no way of raising the needed money. Some people were thus inclined to take advantage of his generosity and kindness, causing him occasional difficulties or unpleasantness. But he did not let this influence his future course. When, only a few months before he died, in 1939, I told him that he ought to write a book about his life, his activities, and philosophy, he chuckled and said: "Yes, I could write several books, but the one I would really like to do would be about the 'Lame Ducks' and 'Ugly Ducklings' I have had around me." I know that he would never have written such a book, no matter how long his life.

He lived in a strange environment. I often wondered what he had in common with the people around him, with whom he swapped stories, played cards, or discussed the problems of the day. He was so gentle that, no doubt, he was easily hurt, although he never showed it. He always kept his good humor.

He was willing to back anybody who trusted in him. When on one occasion, I asked him why he allowed a certain person, completely unfit for his job, to continue holding an important position, thus preventing the hiring of a good man, he almost lost his temper: "What do you expect me to do, throw him out on the street? I never do such things." A few months later, however, when another person, better qualified, came looking for a position, he managed to find a place for him, in spite of the limited funds available.

How often would he call me down to his office and ask solicitously about my work and my financial position! Although he seldom offered advice unless asked, he was always ready to give it when requested. When I received the first award for my scientific work in 1929 (Nitrate of Soda Nitrogen Research Award) at a meeting of the Agronomy Society, which he had attended, he announced the good news to me upon his return the following morning with far greater emotion than I myself experienced at that moment. He was not only proud, he was profoundly touched, certainly far more so than when the award had come to him personally the year before. Incidentally, he had given that money to help establish a scientific foundation.

He did not appreciate great music, nor did he become enthusiastic over fine paintings or sculptures or even great natural beauties, as I

had ample opportunity to note during our travels together in Europe in 1924. What he did observe was the life of the common man, and he was most anxious to do what he could to help improve his lot. He utilized his travels not to visit museums and cathedrals, as many tourists do, but to study methods of farming, problems of food distribution and marketing, and organization of co-operatives. He thus returned home ready to share with his home folk the observations he had made, giving them the benefit of his own conclusions as applied to American life and conditions.

Dr. Lipman was a man of the people, no matter who they were or where they were. He typified the people and was as far from being an intellectual snob as any intellectual could be, without making others feel that he was in any way different from them. He was a great man. It was a privilege to have known him. It was a pleasure to have been able to work for him. Life somehow had a brighter luster because of him. There are not many like him in this world. When he passed away, many of us lost a great teacher, a good friend, and the finest spirit known to man.

"He was the last of the great American Experiment Station directors," recently remarked Dr. William Slate, former director of Connecticut, who knew Dr. Lipman well. To say that of one who was once a poor immigrant boy is to say much. He left his stamp on American life and culture. It is to pioneers like him that America owes its greatness.

At an intimate gathering of a group of Boston scientists at the home of Dr. A. L. Sacher, president of Brandeis University, I was asked to classify my lectures. After some discussion, it was decided to divide them into three categories: (a) original lectures, or those that would be delivered before large scientific bodies, such as national or international congresses; (b) professional lectures, before medical, chemical, or microbiological societies; (c) cultural lectures, before gatherings of intelligent lay people, like student bodies at a university, various service clubs, regional associations for tuberculosis, or similar organizations.

The first type of lecture would be, by its very nature, specialized in scope; each had to be prepared for a particular purpose, based upon

original data or presenting new ideas in a special field. Such a lecture was usually published. I could give few of those.

The second type could be repeated as long as my patience endured. I may cite, as an example, a lecture given in 1950 on the subject of "Searching for Antibiotics," which I delivered before several medical audiences in New York and other American cities, in Madrid and other Spanish cities, in Rome, in Toulouse, and elsewhere. Although I gave it in three different languages (English, Spanish, French), I used the same illustrations, with apparently equal success. In 1952, I delivered the same three lectures before Notre Dame University, in Israel, and later in Japan.

The third type of lecture was in a class by itself. Here, I would invariably study my audience before I definitely decided upon the subject of my address and the illustrations which I was to use. I never talked down to an audience, whether it was made up of a group of high-school students or a large lay body. I had a healthy respect for them all, but I selected, from my vast experience in the field of antibiotics and microbiology, those instances or phases of the subject which not only would appeal to them but also help them understand the more complicated aspects of the subject that I tried to elucidate. I believe I was successful in this. Had I for one moment doubted it, I would have ended it all quite abruptly.

On one of my visits to a southern American city, where I attended an important conference on streptomycin, I overheard two gentlemen, both prominent medical authorities, discuss the subject for the next regional meeting which was to be held there in the near future. One said: "What type of subject would you suggest for this meeting?" "Why not antibiotics?" said the other. "There is still a lot of interest in this field." "Don't you think that this subject is beginning to wear rather thin?" replied the first. I was not at all amazed at such sheer ignorance of the significance of this new field of science. I could well understand this attitude. New "miracle drugs" were being discovered, one might say daily. Most of them were being acclaimed by certain industrial groups, by various medical speakers, and by unscrupulous "band wagon" adherents, as "new" and "great" and "revolutionary" discoveries. Unfortunately, even many medical practitioners fell for them; to them a great discovery became commonplace within two or

three years. Has not many a doctor told me how his patients would come to him and demand that he prescribe the new drugs? "Do not give us penicillin or streptomycin or sulfa drugs, these are old and cheap. They cannot be any good. Give us this, that, or the other; these are new and expensive; they must be good. Are they not advertised heavily? Does not a certain popular magazine speak highly of them?" Some of the popularizers, and even some of the speakers dealing with a new field of science, which has been so productive of practical results, have tried to harvest some of the fruit for their own benefit. There was certainly plenty of fruit to harvest; there was more to come. But how many of these writers and speakers fully appreciated the tremendous efforts involved in launching a new antibiotic as an important chemotherapeutic agent. To them it was just a new word, a new miracle. Today penicillin, tomorrow streptomycin, then chloramphenicol, to be followed by the tetracyclines. What next? Those are old already. Give us new ones. Give us better ones. Give us at least something different to prescribe, to talk about. What did they know of the efforts involved, of the bitter disappointments, of the energy expended? For every success, there were literally hundreds of failures.

Very well, one might say, that is all right so far as your relations to the microbes are concerned, but how about your relations with your fellow man? What have you done to justify those? I have tried to analyze these relations in the previous pages, especially as they concerned my colleagues and my fellow scientists throughout the world, my students, and my own family. If I were to grade these, as a teacher should, I am afraid that the records would be very uneven. In some I no doubt succeeded eminently; in others, I failed miserably. I might go into a long discussion of the reasons and analyze these and perhaps try to understand them. There are, after all, various reasons for any human action or conduct, as influenced, of course, by background, environment, and economic competition. In this respect again, man is like the microbe. He responds to these natural forces; they are responsible for his actions. But, then, man thinks; man can partly control, or at least plan, his destiny. But does he? Or is he merely a product of natural forces, just as is the microbe?

To be sure, I tried in every way possible to avoid committee meet-

ings. They simply bored me or even irked me. I refused to participate in many so-called social activities, like gathering funds for worthy causes. There were so many others who were willing to do it, who had the time, who could do it better than Bobili or I. Although we tried to help all we could, we probably did not do our share, or at least others thought that we did not. Why waste time on "council" meetings, on "board of governors" meetings, on "trustee" meetings, when I could accomplish so much more by devoting myself to the study of the microbes, by giving the benefit of my knowledge to others, especially the young eager minds who could carry on where my generation left off!

The best that I can say to justify my possible failures or inadequacies to my fellow men is to say that at least I have tried. I may not always have analyzed my emotions, but I have always tried to analyze, to weigh, the various angles of any problem. I have tried to make a fair, or at least what appeared to be a fair, decision. Occasionally, I have failed, as in my inability to evaluate frequently the purposes of some of those who came to me for help and guidance. When a few such failures were recognized, there developed a tendency to examine too critically even those who were honest, who could rightfully expect such help and guidance. Unfortunately, it is the failures that leave the ill effects. The fair decisions, the proper decisions, are usually taken for granted. But then, is this not the fate of all men? Have not even the wisest of them made mistakes, experienced failures? Has this not been the reason why so many have left their fellow men and have gone off to the wilderness to be alone, so as not to sin, so as not to fail in their relations with nature or their fellows? Have some not become so discouraged as to say "*Evel avoulim, akol evel*" (*Vanitas vanitatis, omnia est vanitas*)? To live in a human society is to make errors, and thus failures. But then, should we not take these in the same spirit as our successes? At least, in looking back over a complex life, the thing that justifies one's life is to be able to say: "I have tried my best." And that I can truthfully say.

I hope, therefore, that those who have read these pages will not think of me as vain, as desirous of glory or of acclamation. Far be it from me to expect that or even to want it. I would be most happy to

carry away a feeling of having done something, of having advanced even a small step our knowledge of the microbes, or the smallest of living things.

Let, therefore, this humble story of the life of one who has devoted himself to the study of nature and natural processes serve to remind those who chance to read these lines that truth is universal, that man is more than a mere collector of facts or fancies, that the human spirit is continuous and universal, that the lowest abodes may hide treasures as promising as the great palaces, that man, if given the opportunity, will identify himself with the spiritual and cultural values which have made him what he is and which distinguish him from other creatures of nature.

Postscript—What of the future?

It is not surprising that every accomplishment opens new horizons. Just as every new embryo points to a new life, to many new generations, different, frequently better, and often, unfortunately, worse than the previous one, so it is with every new scientific contribution. What of the future? My own, that of the institution which I have initiated and which has been made possible through the labors of our small group working away in the Experiment Station laboratories, where I began my scientific work in 1914, as a senior in college, and which I left forty years later.

With the new institute now completed and occupied, many new problems have arisen. Professor Kluyver, the worthy successor of Prof. Beijerinck in Delft, Holland, said that to do research we must have "bricks and brains." We now have the bricks. But we must have the brains. This will not be an easy problem. We may even go a step further and say, How about the vision, the imagination, the drive? Who will supply those? At this stage of the development, there is needed a unifying force, which will point a way to the future.

It is dangerous to plan too far. This must be left to the others who will come. The point is how to find those who are inspired by the spark of an idea, who are looking not only for jobs or are satisfied in the jobs they hold, but also for purposes, for service, for a better future?

One often becomes discouraged as one observes many of the selfish interests, of the personal selves who are unwilling to give up the least thing, who are unwilling to serve, who want only to grab for themselves. But then come to mind the faces of all the children and grown-ups, the countless letters from the sufferers and their dear ones, who have benefited and who continue to benefit. Is it not worth while, for them alone, to put up with it all, to forget the selfish interests and to hope that in the end the good will benefit and survive, that throughout this continuous struggle for survival of the human race, through countless generations, progress will continue to be made, that we may still look forward in the end to a better world, a world where ills, both human and social, will be controlled, and where man, the best of man, will be the conqueror?

*I would like to end this volume with a bit of poetry
suggesting the infinitesimal part played by man in this
universe.*

A SPECK OF DUST

*Who am I?
But a speck of dust,
Among myriads of particles
Living and dead,
Past and present,
That have come and gone,
In eons of time.*

*Who am I?
But a flicker of light,
Among a universe of beams,
That rush along,
Among paths unknown,
To leave no trace
Where others may follow.*

*Who am I?
But a bundle of electrons,
That gives life to
The speck of dust,
That the light may flicker
And others may come,
To take their place.*

*Who am I?
But a human thought,
That flashes through
The worlds of time,
That specks of dust
May thus become
Giants in this universe.*

*It is the speck of dust,
The flicker of light,*

A Speck of Dust

359

*The bundle of electrons,
The human thought
That have unraveled the universe,
That have unfolded its secrets,
That have seen myriads
Of still smaller specks
Being born and lost to eternity.*

*It is this speck of dust
That watched the even smaller specks
Flicker through and add
Their tiny share,
To the eternity of life,
So that new specks of life
May grow again,
Into the endless stream of life.*

*I am that speck,
From the beginning of time
To the end of time,
That tiny speck in eons of time,
That flicker of light
That has left its trace
Upon the course of life,
The significance of which
In this universe of time
Is still to be unraveled.*

SOME BASIC TECHNICAL TERMS
USED IN THIS BOOK

Actinomyces—A microbe belonging to the actinomycetes.

Actinomycetes—A group of microbes, filamentous in nature; in size and physiology they are more closely related to the bacteria; in structure they are similar to the fungi; may be considered as intermediary between these two groups of microbes.

Algae—Microscopic forms of life containing green chlorophyll.

Antibiotic—A chemical substance produced by a microbe which has the capacity, in dilute solution, to inhibit the growth and even to destroy other microbes.

Arsenic—A chemical element, highly poisonous to animals, but, in chemical combination, frequently used for the treatment of certain infectious diseases.

Aspergillus—A group of fungi or molds of common occurrence on food-stuffs, clothing, and soil and frequently used in industry for the preparation of certain organic acids and other valuable products.

Bacillus, bacilli—A group of bacteria; scientifically those bacteria which produce spores; popularly, any bacteria are often spoken of as bacilli.

Bacterium, bacteria—Microscopic forms of life which multiply by simple division; the individual form divides into two parts, each of which grows into a complete organism.

Biochemical—Chemical study of living processes; application of chemical methods to the study of the composition and different reactions in living systems.

Broth—A nutrient liquid medium prepared from simple chemical substances or complex materials for the growth of microbes.

Carbon—A chemical element; important constituent of all living matter

and of all organic and certain inorganic materials; when combined with the elements hydrogen and oxygen, it gives carbohydrates (sugars, starches, celluloses), or hydrocarbons (oils, inflammable gases).

Cellulose—A chemical substance which is the major constituent of natural fibers, such as cotton, flax, wood pulp, etc.

Chemotherapy—The use of chemicals or drugs for the treatment of human and animal diseases.

Cryptogamic botany—The study of lower forms of plant life, notably fungi, actinomycetes, bacteria, and algae.

Culture—The growth of a microbe in an artificial medium; pure culture—a single type of microbe; mixed culture—one in which more than one type of microbe is present.

Decay—A term occasionally used to designate a certain type of destruction of plant and animal residues by microbes.

Ecological—The relation of organisms to their environment.

Emetine—A chemical extracted from ipecac root used in the treatment of dysentery.

Enzyme—A chemical substance produced by living systems which hastens chemical reactions; amylase hastens hydrolysis of starch to sugar; protease hastens hydrolysis of proteins.

Fermentation—Decomposition of organic materials by microbes, frequently resulting in the formation of gases.

Fixation of nitrogen—Conversion of gaseous atmospheric nitrogen into inorganic or organic forms, thus making it available for plant growth.

Fungi, or molds—Filamentous microbes, free from chlorophyll, larger in size than bacteria and actinomycetes; usually multiplying by sexual reproduction.

Genus—A special group in a natural system; orders and families of animals, plants, and microbes are made up of genera; a genus is made up of species.

Humus—Decomposed organic residues where original plant or animal structure can no longer be recognized; dark in color; present in composts, soils, peat bogs, sea and lake bottoms.

Legumes—A family of plants, including alfalfa, clover, peas, and beans, which form nodules on their roots; these nodules are caused by specific bacteria which enable these plants to use the gaseous atmospheric nitrogen.

Lysis and to lyse—To dissolve the living cell by enzymes or by special viruses.

Mycologist—A student of fungi.

Mycorrhiza—Literally fungus root; an association of a fungus with the root system of plants; essential in the growth of a number of higher plants, notably orchids and evergreens.

Nitrification—A process whereby ammonia is converted in soil or in the sea to nitrate by special groups of microbes.

Nitrogen—A chemical element present as gas in atmosphere; essential constituent of proteins and other compounds found in plants and animals; present in soil and in water in different organic and inorganic forms.

Oxygen—An essential chemical element, making up eighty per cent of the gases of the atmosphere; necessary for breathing and respiration; essential constituent of organic matter.

Parasite—A microbe growing on another living system and causing its destruction.

Penicillium—A group of fungi or molds of common occurrence in nature; used in the production of cheeses; in fermentation industries; for the production of penicillin.

Protein—Essential constituent of all living matter; used in human and animal diet, in the form of meat, milk and its products, beans, and peas.

Proteolytic—Property of microbes to destroy proteins.

Protozoa—A group of microbes characterized by motility and a mode of nutrition that places them closer to the lower animal forms of life.

Putrefaction—Destruction, by microbes, of proteins in the absence of air; characterized by formation of bad-smelling products.

Retting—Treating of flax, hemp, and other fibrous materials in such a way as to favor the development of microbes which separate the fiber from the stem.

Species—*See* Genus.

Substrate—A natural or artificial material in which living organisms are able to grow.

Sulfa drugs—Chemical compounds containing sulfur and used in the treatment of various infectious diseases.

Sulfur—A chemical element present in all plant and animal tissues (in

the protein fraction) and in certain natural springs, mines, and other reservoirs.

Streptomyces—A group of actinomycetes which, next to penicillia, are the most important producers of antibiotics.

Virus—The smallest forms of life, not even seen by ordinary microscope, and living parasitically upon plants and animals.

Vitamin—A chemical substance produced by a living system which is essential for the growth of other living systems, especially animals and man.

About the Author

BORN IN Russia on July 22, 1888, Selman Abraham Waksman chose biology in preference to his father's first choice of industrial chemistry. While planning to study medicine, after emigrating to the United States in 1910, he changed to microbiology and enrolled in the College of Agriculture at Rutgers University in New Jersey. Following graduation in 1915, he earned his Ph.D. at the University of California in 1918 and returned to Rutgers as a teacher and research worker, dividing his time between the classroom and the laboratory.

Since the discovery of streptomycin in 1943, Dr. Waksman has received honors from many nations and universities, reaching the highest pinnacle of scientific achievement with the award of the Nobel Prize for Physiology and Medicine. The new Institute of Microbiology at Rutgers University was built with royalties from the sale of streptomycin and assigned to that University by Dr. Waksman.